and trying this magnificent instrument shortly before its despatch to its owner Mr. Newall, speaks in the highest terms of its mounting and mechanical arrangements generally; and so far as he was able to judge of its optical performance, with the low power to which the state of the weather restricted him, he considered it to be very promising.

The difficulties attending the constructing and mounting of so large an object-glass are exceedingly great. That all these then have been entirely overcome no one conversant with the refinements involved can reasonably expect, and the optical and mechanical perfection of the famous Cooke factory has raised the standard by which telescopes are judged. We have every hope that this instrument will be worthy of their fame.

This record of enlightened private enterprise would be incomplete if we did not add that Mr. Newall proposes, when the instrument is properly fixed in a good climate, to place it at the disposal, for a certain number of hours in each day, of any qualified astronomer who desires to use it. The benefit likely to be rendered to science by Mr. Newall's liberality can hardly be overrated. We understand that his observatory is to be under the direction of Mr. Albert Marth. We have not yet heard that its site has been finally decided on; it is certain that the full powers of such a telescope cannot be called forth in the climate of England.

Mr. Buckingham's Telescope.

In connexion with this splendid accession to observational astronomy, we would call attention to the existence of a telescope not much inferior in size to that just described, which was constructed some years ago by Mr. J. Buckingham, a Fellow of this Society, and which was exhibited by him at the International Exhibition of 1862. Its clear aperture is $21\frac{1}{4}$ inches, and its focal length $28\frac{1}{2}$ feet. It is mounted as an equatoreal of the (so called) German form. Mr. Buckingham has also constructed another telescope of 9 inches aperture, mounted equatoreally. Competent judges, who have been permitted to visit Mr. Buckingham's Observatory at Walworth, speak in the highest terms of the completeness and ingenuity of the arrangements. It is understood that Mr. Buckingham has employed a method, peculiar to himself, of practically correcting the aberrations of the large object-glass. Regarding its optical performance we yet no definite report. Mr. Buckingham is known to be a mechanician of great originality, but also an indus- erver. It is very much to be desired that he should find om his arduous occupations to give to the world a com-

plete account of his optical processes, of the contrivances peculiar to his instrumental mountings, and of the performance and produce of the magnificent apparatus which he has created with so much labour.

Zenith Sectors for Indian Survey.

One of the zenith Sectors constructed for the Great Trigonometrical Survey of India, by Messrs. Troughton and Simms, from Colonel Strange's designs, has been despatched, and has reached its destination, Bangalore, in the Madras Presidency. It went out by the Overland route under charge of Lieut. Rogers, R.E., an officer of the Survey, and is said to have sustained no injury in transit. This instrument has a telescope of 4 inches aperture and 4 feet focal length. The sectors are portions of a circle 3 feet in diameter; they are read by four micrometer microscopes, illuminated by a simple light somewhat in the manner employed in the transit-circle of the Royal Observatory. It is furnished with the appliances of a zenith telescope, and can be used, therefore, as such if desired. The instrument is to be employed in the observation of latitudes on the southern portion of the great meridional arc of India at short intervals, with the twofold object of investigating the very interesting physical question of local attraction, and of eliminating the effects of such attraction on the geodetic determination of the figure of the Earth. These operations have been intrusted to Capt. J. Herschel, R.E.

Mr. Carrington's Observatory.

All who are interested in Astronomy will be glad to hear that our Fellow, Mr. Carrington, whose valuable observations of circumpolar stars and of Sun-spots are well known, is now engaged in erecting a new observatory at Churt, near Farnham. This observatory is arranged on a new plan of construction. As it is situated on a conical hill, entirely detached, and 60 feet high, no elevation of the building is needed. Mr. Carrington has sunk the observatory below ground, so that the instruments can just be directed over the soil at the top of the mound. There has been sunk a dry well, 6 feet in diameter, to a depth of 40 feet, in which a clock, placed in an air-tight case, may be kept at a position of invariable temperature and at a constant and diminished pressure. The observatory will be furnished with an Altazimuth on Steinheil's principle, in which the horizontal axis is also the optical axis; an object-glass of 6 inches, provide with a prism fixed outside it, is placed at one end, and the ey piece at the other end of the axis.

O/MA
Poyal

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1869, *TO JUNE* 1870.

VOL. XXX.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS OF THE ROYAL ASTRONOMICAL SOCIETY.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *November* 12, 1869. No. 1.

WARREN DE LA RUE, Esq., Vice-President, in the Chair.

Lieut. W. H. Collins, R.E.;
Rev. John Edwards; and
Rev. Alphonso Matthey,

were balloted for and duly elected Fellows of the Society.

Solar Eclipse of August 7th, 1869.
By R. T. Paine.

This eclipse was carefully observed by me at the Court House in Boonesboro, Boone County, Iowa, with my small telescope of 2¼-in. aperture, power of 30, erect eye-piece, deep green screen. The latitude, $42^\circ\ 3'\!\cdot\!4$, was determined by me, by a few altitudes of *Altair*, which agreed well with each other, but *Polaris* was too faint to be used. The longitude was ascertained by my two chronometers, by comparison with the clock of the Observatory at Chicago, to be $6^h\ 15^m\ 34^s\!\cdot\!97$; $25^m\ 7^s\!\cdot\!64$ going to Boone, and $9^s\!\cdot\!0$ returning from it to Chicago, which is in long. $5^h\ 5^m\ 26^s\!\cdot\!65$ west from Greenwich. Both latitude and longitude of Boone are, therefore, considered as pretty well known.

The observations were good and the sky clear, although there was some haze.

The corona was good, but, independently of the red or rosy

flames, not, I think, as striking or magnificent as the one at Beaufort, S.C., on November 30th, 1834; moreover, the darkness was *not* as great. I used a lantern on both occasions to read off the chronometers at the second and third contacts, but it was *not* necessary on August 7th. As soon as the second contact took place at Boone the corona was seen, and then a *deep red* flame, a little to the left of the lowest part of the Moon, which remained quite steady to the third; two or three seconds later, two flames of a beautiful rose colour appeared on the left and upper part of the Moon, and continued upwards of a minute: as the Moon moved on, three or four *rosy* flames appeared on the right, which, just before the third contact, seemed to run together. About five minutes before the second, a brush of light descended from the upper crescent of the Sun, and at the *same time* a small part of the lower crescent was broken off, yet nothing of the kind was noticed after the third. The edge of the Moon was generally very sharply defined, but slight irregularities were seen before the second contact, of very short continuance. The red or rosy flames were so conspicuous that they were seen at once, without the assistance of even an opera-glass; but at Beaufort nothing of the kind was noticed, although α *Scorpii*, then (Nov. 30th) in conjunction with the Sun, and therefore only $4\frac{1}{3}^{\circ}$ distant, was seen as soon as the corona. This eclipse will return for the second time on December 22nd, 1870, total at Cadiz, Gibraltar, Syracuse, &c. At Beaufort, lat. 32° 25′ 57″, long. 80° 40′, the interval between the second and third contacts was $1^{h}\ 49^{s}\cdot 6$, and the eclipse was quite central.

The total eclipse of June 16th, 1806, which occurred *here*, central, about half-an-hour before noon, was the finest in the United States in the nineteenth century. The duration of totality at Boston and at our neighbouring city, Salem (where it was observed with great care by the late Dr. Bowditch), was *five* minutes. The excitement about it was great indeed, yet no one saw any red flames, and Dr. Bowditch never gave the least hint that he saw any; and a very intelligent gentleman of Lynnfield, who saw that eclipse in that town, and distinctly recollects the phenomena attending it, and the last eclipse at Springfield, Illinois, tells me he is positive the darkness in June 1806 was much deeper, and that there were no flames, as he certainly must have seen them, as he easily did at Springfield. Yet at the second and third returns of the eclipse of 1806, in 1842, and 1860, these flames were generally observed. The fourth return on July 29th, 1878, will be central in the United States, in Colorado, Texas, &c., and will therefore, doubtless, be carefully observed by even a greater number than that of August last, although then the number of observers on the central line, or very near it, was not small.

The phases of the eclipse of August 7th, 1869, at Boonesboro, Iowa, lat. 42° 3′ 23″, long. 93° 53′ 45″, were as follows, by strict computation and observation:—

	By computation. h m s	By observation (Mean of two Chrons.) h m s
Beginning of Eclipse	3 40 37·6 M.T.	3 40 46·78
,, Totality	4 43 20·6	4 43 24·48 = +3·88
End of Totality	46 20·6	46 23·28 = +2·68
,, Eclipse	5 43 36·5	5 43 30·18
Duration of Totality	3 0·0	2 58·8

Boston. Oct. 2nd, 1869.

Solar Eclipse of August 7th, 1869. By E. D. Ashe, Commander R.N.

(Extract from a Letter to Mr. De La Rue, dated Observatory, Quebec, August 28, 1869.)

I hasten to communicate to you the result of the Canadian eclipse party that went to Jefferson, Iowa. It consisted of Mr. Douglas, Mr. Falconer, and myself. The funds would only admit of my taking the equatoreal of 8-inch aperture, 9 feet focus, therefore I leave to the American astronomers the description of the results obtained by the spectroscope, &c.

Jefferson is a little to the south of the central line of eclipse. The totality was a few seconds over three minutes. The morning was cloudy; about noon the Sun appeared, and although there were no clouds during the eclipse, still it was hazy.

The total eclipse began at $4^h\ 41^m\ 6^s$, so the Sun was at a very good altitude.

I took several photographs of the partial eclipse, enlarged by an eye-piece to $3\frac{1}{4}$ inches; one shows distinctly a band of light round the limit of the Moon on the Sun. I was afraid of the hazy atmosphere, and altered my arrangements for taking photographs during totality with the eye-piece, and put in the tube fitted for taking the Sun in the principal focus; and had I not done so, the actinic power was so bad that I should have failed completely. In consequence of the change I had no wires to connect the protuberances with the axis of the Sun; but this is of no consequence, as the American party fifty miles to the southward have well determined their position.

I took four negatives* during totality with an exposure of 10^s, and when they are examined with a magnifying glass they are full of information.

* These are now in Mr. De La Rue's care.—Ed.

American Photographs of Total Solar Eclipse of August 7, 1869.
By Rev. T. W. Webb.

Through the courteous intervention of my valued correspondent W. S. Gilman, Jun. Esq. of New York (himself an accomplished observer of astronomical phenomena), I have been favoured by Professor Mayer, of Lehigh University, Pennsylvania, with some specimens of the photographs taken by him during the late total Solar Eclipse of August 7. As these were sent direct from America to Somerset House, I have not had an opportunity of seeing them; but, as I have every reason to believe that they will prove acceptable to the Society, I beg permission to forward to you the following explanatory statement, taken, with some abridgment, from Professor Mayer's letter to myself:—

"The Photographic Expedition, of which these photographs are some of the results, was organized by Prof. Henry Morton, Secretary of the Franklin Institute, Philadelphia, under the authority of Prof. I. H. C. Coffin, U.S.N., the Superintendent of our National Nautical Almanac. The expedition was divided into three parties, stationed respectively at Burlington, Mount Pleasant, and Ottumwa, in the State of Iowa. To me was assigned the organization and command of the station of Burlington, and the photographs I send were taken by me at that place.

Burlington, Iowa, is a town on the W. bank of the Mississippi River, situate in Lat. N. $40^\circ\ 48'\ 21''\cdot 58$, Long. $0^h\ 56^m\ 13^s\cdot 88$ W. of the Observatory at Washington. This station was about 7 miles N. of the centre of the Moon's shadow.

"The telescope, by Merz and Mähler, of 6·42 inches aperture and 9 feet focus, was equatorially mounted, and driven by one of Frauenhofer's friction-governor clocks. The Sun's image, 2·04 inches in diameter, was formed on the plate of the camera by a negative eye-piece specially calculated to give the least aberration. The image of a reticule of two spider-threads at right angles was also projected on the plate with the Sun's disk, and one of these lines was accurately adjusted to the celestial equator; and thus the photographs give precise position-angles of the contacts and of the protuberances.

"A plate having a slot of $\frac{1}{20}$ inch in breadth shot across the eye-piece by the action of a spring for the exposure during partial phase. The duration of this *flash* of the Sun upon the camera-plate I have made the subject of experimental investigation; and I find it to have been almost exactly $\frac{1}{500}$th of a second. A 2-inch aperture of object-glass was used during partial phase work. During totality the full aperture was used, and a slide which allowed the whole beam to fall upon the plate; the exposures varying from 5 to 7 seconds.

"Forty-one perfect photographs were taken during the eclipse, and five of these (all of which I send) were taken during totality, which lasted with us $2^m\ 42^s$. I send nine plates taken at the

times I place opposite the numbers, which correspond with those on the plates.

	No.		Sidereal Time of Burlington. h m s
Before Totality	4	.. ..	13 2 4·1
	19	.. ..	13 49 39·8
	21	.. ..	13 58 32·4
Totality	23	(Exposure 5ˢ)	14 4 1·4
	24	(„ 5)	14 4 29·6
	25	(„ 7)	14 4 52·8
	26	(„ 7)	14 5 10·37
	27	(„ 7)	14 5 40·5
After Totality	35	.. ..	14 34 59·5

"The times were electrically recorded on a chronograph by the exposing plate breaking the electric circuit.

"The photograph numbered 4, taken 2ˢ·8 after *observed* contact, shows a depression in the Sun's limb at the position of first contact, and from this depression shoots into the Sun a high lunar mountain, whose position, measured from the S. point of the cusps, is ¼ of the distance to the N. point of the same.

"You will observe how beautifully defined are the two large spots in the S.W. and N.E. quadrants; the latter surrounded by well-developed faculæ, one of which seems to bridge over the spot and divide it into two portions. I also call attention to the gradation of shade from the border of the Sun inward, the faculæ, the mountains on the Moon's limb, and a glow like that of early dawn (also obtained in the photographs by Mr. De La Rue in 1860), which extends to 18″ beyond the limb of the Moon.

"I could hardly have wished for better success during totality. I might probably have obtained more of the corona by longer exposures, but it would have been at the expense of the fine definition in the prominences, to which my work was specially directed.

"Will you have the courtesy, my dear Sir, to present to the Astronomical Society the nine photographs on glass taken from the original negatives with an orthoscopic lens: I wish them to be preserved in their collection. The other three enlarged copies on paper (also made with an orthoscopic lens) I wish you to receive as a present from me. The best way to examine the glass photographs is to incline them over a piece of white drawing paper placed before a north window, and use a lens magnifying about eight diameters."

Thus far, in substance, Professor Mayer, who has also kindly offered for the Society's acceptance a copy of his Report on the Eclipse, published in the Journal of the Franklin Institute, which contains many additional particulars of much interest. He has also presented to them a copy of his Lecture-Notes on Physics.

Hardwick Vicarage, Nov. 10, 1869.

The November Star-Shower in 1869. By A. S. Herschel, Esq.

The observations of the appearances of the November Meteors, during the last few years of their great returns, present certain irregularities, which it is either possible for physical astronomy to explain, by calculating the perturbations exercised upon the course of the stream by the attraction of the planets, or which may arise from the peculiar configuration of the meteoric current. Supposing the main body of the November meteors, which, in the years 1866 and 1867, made its appearance, successively, in Europe and America, to be an individual portion of the stream, subject during the years 1868 and 1869 to a continuance of the same perturbations which made its return in the year 1867 take place two hours later than the time when (from its occurrence in 1866) it was expected to appear; supposing also that the density of the current, at the point where the Earth will traverse it in the present month, is not yet exhausted,—the time of reappearance of the main-stream of meteors, this year, in England will be almost exactly the same,—between 1^h and 2^h A.M. on the morning of the 14th instant,—as that of its greatest intensity in 1866. A watch for meteors continued from the time of *Leo's* rising on Saturday night the 13th, until sunrise on Sunday morning next, the 14th instant, will probably be rewarded by a view of the return of the central portion of the meteor-stream in Great Britain, although, doubtless, owing to the waning character of the phenomenon, with diminished intensity, yet under the same favourable conditions as those in which it was observed in England in 1866.

Observations at other times and places than those of the great returns of the star-shower in 1866 and 1867 appear to indicate the existence of two meteoric currents bordering upon the main-stream, but separated from it by blank spaces almost entirely devoid of meteors, and forming lateral outliers of the stream, near to which they appear to move in parallel, and closely adjacent orbits. The passage of the Earth through the first of these outlying streams occurs about twelve hours earlier, and its passage through the last about twelve or fifteen hours later, than its appulse with the main or central current. For a knowledge of this peculiar conformation of the November meteoric stream, as exhibited in its occurrences of the last few years, I am indebted to a very important observation communicated to me at the beginning of the present year by Mr. Benjamin V. Marsh, of Philadelphia, U.S., of the occurrence of a remarkably brilliant meteoric shower in China, on the morning of the 15th of November, 1867, *about twelve hours later, in absolute time*, than the beginning and end of the great display seen *in the same year* in the United States.* The times of the previously recorded star-showers, and of that conspicuously seen in America last year,

* *Proceedings of the American Philosophical Society*, vol. x. p. 384.

are thus graphically exhibited by Mr. Marsh in a single diagram (No. 1), which shows the dates and hours of their occurrence, in Philadelphia time, reduced to the common epoch of the year 1868.

Diagram No. 1.

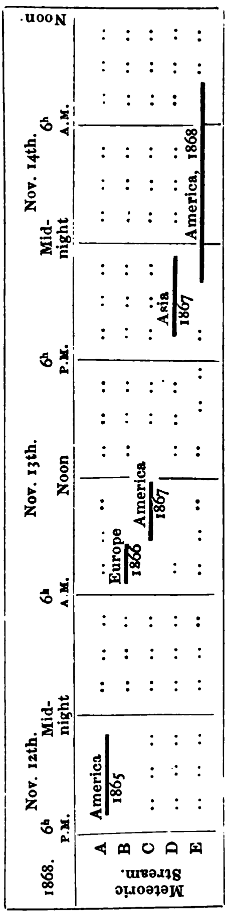
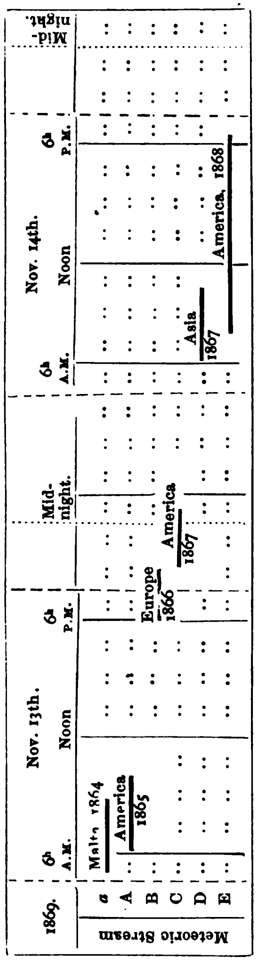


Diagram No. 2.



Place, Year, and Local Time of Maximum.				Philadelphia Time of Ditto.		
			h m			h m
Shower A, Philadelphia,	1865,	Nov.	13, 3 0 A.M.	1865,	Nov.	13, 3 0 A.M.
B, London,	1866,	,,	14, 1 15 ,,	1866,	,,	13, 3 0 P.M.
C, Philadelphia,	1867,	,,	14, 4 30 ,,	1867,	,,	14, 4 30 A.M.
D, Shanghai,	67,	,,	15, 4 30 ,,	67,	,,	14, 3 30 P.M.
E, Philadelphia,	1868,	,,	14, 3 0 ,,	1868,	,,	14, 3 0 A.M.

Philadelphia should have crossed those streams in 1868 (as shown in the diagram), at the following times: —

Meteoric Stream and its Duration. (Philadelphia Time).	Maximum. (Philadelphia Time).
A, 1868, Nov. 12, 7h 0m P.M. to 11h 0m P.M.	1868, Nov. 12, 9h 0m P.M.
B, „ „ 13, 7 0 A.M. „ 9 0 A.M.	„ „ 13, 8 0 A.M.
C, „ „ 13, 9 0 „ „ 12 0 Noon	„ „ 13, 10 30 „
D, „ „ 13, 7 30 P.M. „ 11 30 P.M.	„ „ 13, 9 30 P.M.
E, „ „ 13, 10 0 P.M. „ 8 0 A.M. (Nov. 14)	„ „ 14, 3 0 A.M.

A simple inspection of the diagram will suffice to show that the great shower of meteors seen last year in Europe and America corresponds more nearly, in the time of its appearance, *to the Asiatic star-shower*, than to the American apparition of the meteors in 1867; while the latter appears to coincide most nearly with a reappearance of the main body of the meteors observed in Europe in the previous year.

By adding five hours to the times in the last of the foregoing tables, for the longitude of Philadelphia west from Greenwich, and six hours more for the fraction of a day in one tropical year elapsed since the epoch for which it was prepared, the following diagram (No. 2, p. 7) is easily drawn from that obtained by Mr. Marsh, to aid observers of the shower in England in the present year: —

The shower (*a*) is added to the list from an observation recorded in the "British Association Report for 1865," p. 122; and the times when Greenwich should cross the several streams this year are as follows: —

Meteoric Stream and its Duration. (Greenwich Mean Time).	Maximum. (Greenwich Mean Time).
a, 1869, Nov. 13, 5h 0m A.M. to 9h 0m A.M.	1869, Nov. 13, 7h 0m A.M.
A, „ „ 13, 6 0 „ „ 10 0 „	„ „ 13, 8 0 „
B, „ „ 13, 6 0 P.M. „ 8 0 P.M.	„ „ 13, 7 0 P.M.
C, „ „ 13, 8 0 „ „ 11 0 „	„ „ 13, 9 30 „
D, „ „ 14, 6 30 A.M. „ 10 30 A.M.	„ „ 14, 8 30 A.M.
E, „ „ 14, 9 0 A.M. „ 7 0 P.M.	„ „ 14, 2 0 P.M.

The vertical broken lines in the figure indicate the duration of darkness, and the vertical dotted lines the time when the radiant-point in *Leo* rises above the horizon at Greenwich. Should the main body of the meteors reappear in the present year, without having undergone any perturbations since November 1867, a few meteors of their display will be visible at Greenwich in the last hour preceding midnight on Saturday the 13th instant. If, on the other hand, the same gradual advance of the shower continues, which is perceived on the diagrams in all the three branches of which the stream consists, the reappearance of

the central shower will commence at Greenwich on Saturday at midnight, and continue until Sunday morning, at 3^h A.M.; the circumstances in this case being as favourable for its complete observation as they were on the night of the 13th–14th of November, 1866.

Although it does not appear that the outlying currents of the group are this year favourably situated for observation in Great Britain, yet the observation of a frequency of meteors towards daybreak on the morning of the 13th (Saturday) and of an unusual number of meteors after 11 o'clock P.M. on the night of the 14th (Sunday) would confirm the probable existence of two companion streams of the central meteor-current, and would afford material data for determining the limits of their extent.

The large meteor which was very generally seen in the south of England at about $6^h 50^m$ P.M. on the evening of the 6th instant, was also seen at Hawkhurst, in Kent, and the position of the luminous streak which it left visible, standing, apparently, quite perpendicularly over the point where the meteor disappeared, was noted exactly by the stars. It extended from *Vega Lyræ* to a point two or three degrees above and to the right of the star *μ Herculis*, where the meteor burst into fragments of white blue and orange colours, and left a small cloud of light at that spot, like a nebula, which remained visible for several minutes. The observer particularly noticed the extreme rapidity of the meteor's flight, and its perpendicular direction towards the Earth; but no sound of a report was heard, although he listened attentively for some minutes. The direction of the meteor was nearly due west, and its point of disappearance was about 40° from the horizon.

Note on the Sun's Motion in Space and on the relative Distances of the Fixed Stars of various Magnitudes. By Richard A. Proctor, B.A.

Having recently had occasion to examine Mr. Main's Table of the Proper Motions of 1167 Stars, and the conclusions with reference to the Sun's motion deduced from that table by a method devised by the Astronomer Royal, and carried out at his request by Mr. Dunkin, I have been led to notice certain facts which seem to me to be not without significance.

In the first place, I would call attention to the table drawn up by Mr. Dunkin, in which the sums of the squares of the stars' proper motions are compared with the corresponding sums when the proper motion of each star has been corrected for the Sun's calculated motion in space. Mr. Dunkin comments on the singular smallness of the correction thus introduced. And this view seems abundantly verified by the table, which runs as follows, the divisions 1 to 7 corresponding to Struve's arrangement of the stars in order of magnitude:—

Sums of Squares of Motion in Parallel.

		Uncorrected.	Corrected.
Division	1	2·0637	1·2123
	2	1·8743	1·9292
	3	9·2894	9·5607
	4	6·4732	5·4608
	5	43·4126	42·4236
	6	14·8637	14·2750
	7	0·7814	0·7215
	Sum	78·7583	75·5831

ums of Squares of Motion in N.P.D.

		Uncorrected.	Corrected.
Division	1	6·7231	5·6883
	2	0·5351	0·7805
	3	4·9739	4·7569
	4	6·9390	6·5255
	5	39·4335	38·7292
	6	4·5671	4·3376
	7	0·0951	0·0904
	Sum	63·2668	60·9084

Commenting on this result, Sir John Herschel remarks:—
"No one need be surprised at this. If the Sun move in space, why not also the stars? And if so, it would be manifestly absurd to expect that any movement could be assigned to the Sun by any system of calculation which would account for more than a very small portion of the totality of the observed displacements."

It had always seemed to me that this conclusion might require to be modified if the question were subjected to mathematical scrutiny; my reason for forming this view being this,—that the largeness of the number of stars operates as much to increase the extent of the correction as to increase the amount of the uncorrected squares, since every star is affected by the Sun's motion in space.

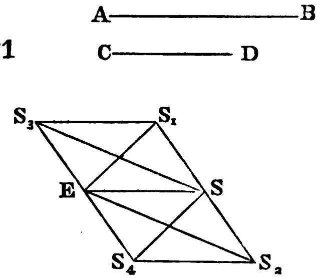
It occurred to me recently that the following simple geometrical proof serves to show that the correction to be looked for is much larger than that which Mr. Dunkin's figures exhibit.

Suppose that in any small region of the sky there are a large number of stars (say n), all at the same distance from the Earth, and travelling in all directions with a velocity exactly equal to that with which the Sun is travelling. Let us suppose that the motion of any one of these stars which is travelling at right angles to the line of sight carries the star over an arc p in a year; and that the region of the sky is so situated that the effect of the

Sun's motion on a star at rest (at the given distance) would be to produce an apparent annual proper motion q'.

Let $A B$ (fig. 1) $= p$
$C D \quad = q'$

Fig 1



Suppose two of the stars to move in opposite directions, $S S_1$, $S S_2$, along the same straight line, both (we may suppose for convenience) starting from S, and the lines $S S_1$, $S S_2$, being in reality equal to $A B$, but foreshortened. Then the sum of squares for these stars, if unaffected by the Sun's motion, would be

$$= S S_1^2 + S S_2^2,$$

where $S S_1$, $S S_2$, represent the *apparent* lengths of these lines.

Suppose that $S E$ is the apparent motion due to the Sun's real motion, so that $S E = C D$.

Complete the parallelograms $S S_1 S_3 E$ and $S E S_4 S_2$, and draw their diagonals. Then the sum of squares for the two stars as affected by the Sun's motion,

$$\begin{aligned} &= S S_3^2 + S S_4^2 \\ &= S_2 E^2 + S_1 E^2 \\ &= S S_1^2 + S S_2^2 + 2 E S^2 \end{aligned}$$

Therefore, for every pair of stars moving in opposite directions, there is an increase of $2 q'^2$ in the sum of squares. Therefore there is an average increase of q'^2 for each star in the region.

Now a moment's consideration will show that, precisely as the lines $S S_1$ and $S S_2$ vary from the full length $A B$ to zero, as we vary the supposed motion in all directions around S (for stars in a given small region of the heavens), so does $C D$ vary from $A B$ to zero as we shift the small region over the heavens. It is not merely that the limits of change are the same, but the proportion of lines of a given length $S S_1$ for the star's motion is exactly the same as the proportion of corrections equal to $S S_1$ for the Sun's motion. Since, therefore, for each region there is an increase per star equal to the square of the proper motion due to the Sun's motion (for that region), and the several stars of each region have proper motions varying according to exactly the same law as that according to which the effect of the Sun's motion varies over the celestial sphere, it is obvious that for the whole sphere the effect of the Sun's motion must be to increase the sum of squares by an amount exactly equal to the sum of squares due to the stars' own motions. In other words, the sum of squares is doubled

through the effect of the Sun's motion, if only the Sun be assumed to travel at the same rate through space as the several stars at a given distance from him.

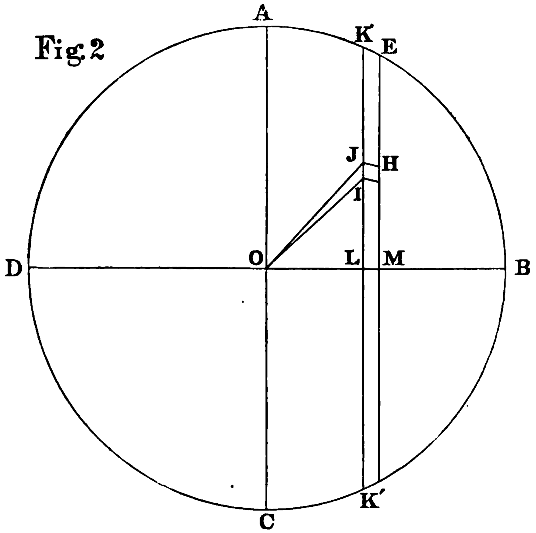
It is obvious also that the same is true if the stars at a given distance move with different velocities, but the Sun's velocity is the mean (its square equal to the mean of sum of squares) of the velocities of the stars at said distance.

Further, if the mean velocity of the stars at a given distance be p, and the Sun's velocity be q, then we have the ratio

$$\frac{\text{Corrected sum of squares}}{\text{Uncorrected sum}} = \frac{p^2}{p^2 + q^2}.$$

Before proceeding to apply this law, I propose to deduce it by another process, which will serve to indicate what is the actual sum of squares, corrected or uncorrected, for a given large number of stars, all at the same distance from the Earth.

Fig. 2



Let A B C D, fig. 2, represent a small circular space on the celestial sphere; and suppose a large number of stars within this space, travelling in all directions with a mean velocity which would give to one moving at right angles to the line of sight an annual proper motion $= p$. Then, in order to determine the mean square of the apparent proper motion — the Earth being supposed to be at rest — we may suppose every star to start from O, O B $= p$, and the points towards which the stars are severally moving, uniformly spread over the sphere A B C D.

Take now a thin zone of the sphere's surface by parallel planes perpendicular to O B, through L K, M E, where

$$OL = x, \qquad OM = x + \delta x.$$

Then the number of stars whose directions lie towards some part of this zone, is (by a well-known property of the sphere),

$$= \frac{n \,.\, \delta x}{2p},$$

n being the total number of stars.

Again, take planes B O I, B O J, inclined at angles θ and $(\theta + \delta \theta)$ to the plane through O B and the observer's eye.

Then the number of stars, whose direction is towards some part of the small element I H of the sphere's surface,

$$= \frac{n \,\delta x}{2p} \cdot \frac{\delta \theta}{2\pi}.$$

When the element I H is taken small enough, each of these stars has an apparent proper motion, whose square

$$= x^2 + (p^2 - x^2) \sin^2 \theta.$$

Therefore, it follows that the mean value required

$$= \frac{1}{p\pi} \int_0^{\frac{\pi}{2}} \int_{-p}^{+p} \left\{ x^2 + (p^2 - x^2) \sin^2 \theta \right\} d\theta \, dx$$

$$= \frac{2p^2}{\pi} \int_0^{\frac{\pi}{2}} \left(\frac{\cos^2 \theta}{3} + \sin^2 \theta \right) d\theta$$

$$= \frac{2p^2}{3\pi} \int_0^{\frac{\pi}{2}} \left\{ \frac{1 + \cos 2\theta}{2} + \frac{3(1 - \cos 2\theta)}{2} \right\} \delta \theta$$

$$= \frac{2p^2}{3}; \qquad (1)$$

and the sum of squares is therefore

$$= \frac{2np^2}{3}. \qquad (2)$$

Next let us inquire what the sum would be if the solar system were moving in such a manner that each star within the circle A B C D was affected by a proper motion q (in any direction) due to the Sun's motion.

Let the motion be parallel to O B, and from right to left. Then it is clear that the only change in the expression to be integrated, is, that for x^2 we must write $(x - q)^2$. Therefore the mean square will be

$$\frac{2p^2}{3} + \frac{1}{p\pi}\int_0^{\frac{\pi}{2}}\int_{-p}^{+p}(q^2 - 2qx)\,d\theta\,dx$$

$$= \frac{2p^2}{3} + q^2. \qquad (3)$$

In this case, then, the sum of squares is

$$= \frac{2np^2}{3} + nq^2. \qquad (4)$$

Lastly, we have to inquire what is the sum of squares for N stars scattered over the whole of the celestial sphere at a given distance R, each affected by the proper motion *p in space*, the Sun being affected by a motion P (p and P representing the arc-motions due to these respective proper motions, when supposed to be taking place in a direction at right angles to the line of sight, and to be seen from a distance R).

In fig. 2, let A B C D now represent the celestial sphere, and take

$$\mathrm{OL} = y$$
$$\mathrm{LM} = \delta y;$$

then the effect of the Sun's motion upon all stars in the zone KMK′ will be to affect them with a proper motion,

$$\mathrm{P}\sin\cos^{-1}\left(\frac{y}{\mathrm{R}}\right)$$

$$= \mathrm{P}\sqrt{\left(1 - \frac{y^2}{\mathrm{R}^2}\right)}.$$

Hence, by (2) the average proper motion of stars in this band

$$= \frac{2p^2}{3} + \mathrm{P}^2\left(1 - \frac{y^2}{\mathrm{R}^2}\right);$$

and, therefore, for the whole sphere, the average proper motion,

$$= \frac{1}{2\mathrm{R}}\int_{-\mathrm{R}}^{+\mathrm{R}}\left\{\frac{2p^2}{3} + \mathrm{P}^2\left(1 - \frac{y^2}{\mathrm{R}^2}\right)\right\}dy$$

$$= \frac{2p^2}{3} + \mathrm{P}^2 - \frac{\mathrm{P}^2}{3}$$

$$= \frac{2}{3}(p^2 + \mathrm{P}^2). \qquad (5)$$

and the sum of squares of proper motions

$$= \frac{2N}{3}(p^2 + P^2) \qquad (6)$$

If $P = p$ (that is, if the Sun's motion be assumed equal to the average motions of the stars) the sum of squares,

$$= \frac{4N}{3} \cdot p^2. \qquad (7)$$

The use of the integral calculus, as above, would only be justified where N is infinite; but where N is considerable, the result must be a close approximation to the truth, with the assumed conditions. And even where N is small, the above result is the most probable, in the case of a random distribution of the N stars, and of the directions of their motion.

Now, if we apply these results to the tables given above, we can determine how far the observed proper motions of the stars are consistent with the supposition that the Sun's proper motion is not very different from the mean proper motion of the stars of different magnitudes; and thence we can form an opinion as to the justice of those estimates of the stars' distances on which the values of the corrections tabulated above have been determined.

First, we require the sums of squares of proper motions, without reference to direction. Since the square of a star's proper motion is equal to the sum of the squares of the star's proper motion in parallel and in N.P.D., we have only to add the respective sums of squares in the two tables given above, to deduce the following table:—

Sums of Squares of full Proper Motion.

	Uncorrected.	Corrected.	N.
Division 1	8·7868	6·9006	9
2	2·4094	2·7097	55
3	14·2633	14·3178	146
4	13·4122	11·9863	238
5	82·8461	81·1528	330
6	19·4308	18·6126	368
7	0·8765	0·8119	21
Sum	142·0251	136·4917	1167

Let us first apply formulæ (2) and (6), (remembering that formula (2) is true for the whole celestial sphere). We have from them,—

$$\text{Uncorrected sum of squares} = \frac{2N}{3}(p^2 + P^2),$$

$$\text{Corrected sum of squares} = \frac{2N \cdot p^2}{3}.$$

Hence

$$P^2 = p^2 \left(\frac{\text{correction}}{\text{corrected sum}}\right).$$

Applying this successively to the several divisions, we obtain

For Division 1	$P = p$ (·52)
2	P has an imaginary value
3	P has an imaginary value
4	$P = p$ (·34)
5	$P = p$ (·18)
6	$P = p$ (·21)
7	$P = p$ (·28)

The results for the second and third divisions suffice to show that the numbers of stars which fall under these heads are insufficient for a satisfactory determination; and therefore, *à fortiori*, the number of stars in division 1 is insufficient. Hence the result $P = p$ (·52) must be dismissed as valueless. But even at this stage of the inquiry we begin to recognise that there must be something wrong about our assumptions. For even if the numbers 55 and 146 were not in themselves large enough to lead us to expect a satisfactory evaluation for stars in the second and third divisions, yet the fact that the Sun's motion in space has been correctly deduced from a smaller number of stars would justify such an expectation.

I should be led then to suspect from this evidence, that if divisions 1, 2, and 3, had been taken together, a more satisfactory conclusion would have been arrived at, notwithstanding the apparent necessity of assigning different distances to stars in these divisions. However, it is not possible to determine how far this suspicion is justified without going over Mr. Dunkin's labours afresh, with changed assumptions; and I have no leisure for the long processes of calculation this would involve.

Next we may notice that the results for divisions 4, 5, 6, and 7, are very far from satisfactory. I cannot think it credible that the real solution of the difficulty involved in the smallness of the observed corrections is to be found in the assumption that the mean motion of stars of the fourth magnitude is three times as great as the Sun's, the mean motion of fifth-magnitude stars nearly six times as great as the Sun's, and so on.

If we try the effect of diminishing the assumed distances of stars of the 4th, 5th, and 6th divisions, so as to accord with the observed relations, on the assumption that in reality $P = p$, we have (the assumed distances being 3·76, 5·44, and 7·86,* respectively) the following results:—

* Struve's values.

For Division 4	Distance	$= 3\cdot76 \times \cdot34 = 1\cdot28$
5	,,	$= 5\cdot44 \times \cdot18 = 0\cdot98$
6	,,	$= 7\cdot86 \times \cdot21 = 1\cdot65$

As division 7 includes but 21 stars, we cannot expect any trustworthy results from treating it in the same way.

The evidence thus far seems strongly opposed to accepted views respecting the distances of stars of the smaller visible magnitudes. A further inquiry on this point seems, therefore, suggested. And it is obvious that in the above table of sums of squares we have the means of testing how far the assumed distances of the stars of various orders accord with the observed proper motions. I make the assumption that the Sun's motion is equal to the average proper motion of the fixed stars. This assumption affects the actual, but not the relative, values of the distances which result from the following processes.

We have then formula (7) to deal with. Applying it to the successive orders of stars, *i.e.* putting $\frac{4\,N}{3}\,p^2$ successively equal to the values tabulated in the foregoing columns of uncorrected sums of squares, we obtain,

	Apparent Proper Motion.	Resulting Distance.
Division 1	$0''\cdot857$	1
2	$0\cdot182$	$4\cdot7$
3	$0\cdot268$	$3\cdot2$
4	$0\cdot208$	$4\cdot1$
5	$0\cdot433$	$2\cdot0$
6	$0\cdot191$	$4\cdot5$
7	$0\cdot173$	$5\cdot0$

This result is unsatisfactory in the extreme. We find stars of the second magnitude placed (according to this mode of estimating their distances) further from us than stars of the 3rd, 4th, 5th, and 6th magnitudes.

Remembering the evidence we have already had, that (1) there is something erroneous in our assumptions respecting star-distances; and secondly, that small numbers of stars are insufficient for our guidance, let us apply a test which there ought to be no mistaking. Let us divide the stars into two sets, the first including divisions 1, 2, 3; the second, the remaining divisions and let us apply formula (7) to these sets.

We obtain,

For Set 1, Apparent Proper Motion,

$$= \sqrt{\frac{3}{4}\left(\frac{25\cdot4595}{210}\right)} = 0''\cdot3015,$$

For Set 2, Apparent Proper Motion

$$= \sqrt{\frac{3}{4}\left(\frac{116 \cdot 5656}{957}\right)} = 0'' \cdot 3022.$$

This result would make the mean distance of stars of the first three magnitudes equal to (or *very slightly less* than) the mean distance of stars of the next three magnitudes!

I am very far from supposing that this result accurately represents the relations subsisting among the stars; but I do think that it suffices to render the usually accepted views respecting stellar distribution wholly untenable. Remembering that whatever theory we form regarding the relation between the apparent brilliancy and the real distance of the stars, we must yet recognise the fact that the stars are at very various distances from us. I think it must be admitted that the apparent brightness of a star is, to a certain extent, an argument of relative proximity. A large proper motion is also an argument of relative proximity. If the two indications agreed either for separate stars or for sets of stars, arranged according to apparent brilliancy, there would be no difficulty. As we find, however, that there is no such agreement, we are forced to consider whether brightness or large apparent motion is the stronger evidence of proximity. Judging from the analogy of the solar system, in which the range in the variations of magnitude is enormously greater than the range in the variations of velocity, we seem strongly led to look on the proper motions of stars as in reality the best evidence we have respecting their distances. But this conclusion is very much strengthened when we remember that the dynamical conditions in the sidereal system must be much more unfavourable to the occurrence of wide variations of velocity, than the conditions which prevail in a system of bodies circling around a central body enormously large compared with any of its dependent orbs.

I think, then, that I may fairly look upon the above inquiry as affording very striking evidence in favour of the view I had formed from other considerations, that the assumed estimate of the distances of the smaller stars has been greatly overrated. And as this conclusion may obviously be extended to yet smaller stars, I think that I have not been deceived in looking upon the relations which subsist between the Milky Way and the lucid stars in its neighbourhood as very much more intimate than has been commonly supposed. I believe that future researches will prove, not only that the Milky Way as a whole is much nearer than we have been imagining, but that portions of it are absolutely nearer to us than the brightest of the single stars. That parts of the Milky Way, for instance, in the neighbourhood of *α Centauri* (the nearest of the fixed stars, so far as is yet known), are nearer to us than that star, I think the whole aspect of the galaxy in that neighbourhood suffices to suggest, if not to demonstrate.

Suggestion of a Method of Imitating the Transit of a Planet over the Sun. By the Rev. T. R. Robinson, D.D., F.R.A.S., &c.

While reading Mr. Stone's admirable papers on the transit of *Venus*, it occurred to me that experimental information as to the "black drop," which plays so important a part in this inquiry, may be obtained by means which I formerly used in some researches on irradiation, that were published in the 5th volume of the Society's *Memoirs*.

To obtain an artificial Sun I fixed a plate of brass, in which was a small circular aperture, in the focus of a good telescope. This was viewed, collimator-wise, with another telescope; and when illuminated by a lamp placed behind it, appeared as a luminous disk, $17'$ diameter, sharply defined, and about as bright as the Sun was in the transit instrument. By interposing a piece of oiled paper the disk became much fainter, though the wires in the micrometer of the observing telescope were quite distinct. Making them internal tangents to the disk, and withdrawing the oiled paper, the limbs projected outside the wires from two to three seconds by the effect of irradiation.

Now an artificial planet may be obtained in two ways. 1. A small opaque disk may be carried by the frame of a micrometer in the observing telescope; it will appear as a black spot on the luminous disk, which can be moved at pleasure. Making the illumination of the aperture as faint as is consistent with distinct vision, the circumference can be brought into *real* contact and the corresponding micrometer reading noted. Then, using the full illumination, I expect that the "black drop" would appear, and that an *apparent* internal contact could also be produced. If the arm carrying the opaque disk be supposed likely to interfere with an exact appreciation of the phenomena, it might be supported by a central perpendicular wire an inch or two above the micrometer frame.

2. Or, which seems preferable, the opaque disk may be placed within the aperture in its plane, and moveable in that plane by a micrometer attached to the collimating telescope. This plan has the advantage that both Sun and planet are formed under precisely similar optical conditions, and that the apparent motion of the latter can be more conveniently imitated by attaching a driving-clock to the micrometer-screw.

The precautions which were required to give the aperture an edge which can bear high magnifying, are described in the paper referred to, and they would probably suffice to give an exact metal disk. I would, however, prefer one formed by drilling a conical hole through two pieces of glass cemented together, as described for the aperture; on separating them the edges of the sections which had been in contact would be as smooth as art can make them, and by filling one of them with black cement, a true

disk would be obtained, which could be placed exactly in the plane of the sun-aperture. The glass should be attached to the micrometer-frame, and thus no disturbance of the images by any influence of a support could occur.

If the illumination were made by magnesian or electric light, this apparatus would be also available for Photographic experiments on the Transit.

On the Increase of Probable Errors in a Transit of Venus as dependent upon the Smallness of Normal Velocity. By E. J. Stone, Esq.

In the *Monthly Notices* for April there appears a short paper of mine, "On the Comparative Clinging of the Limbs of *Venus* and the Sun in the Transit of 1874," and some consequences which appeared to follow therefrom.

In discussing the observations made in 1769, I convinced myself that the chief source of error to be feared was erroneous assumption of semidiameter. The errors arising from causes which chiefly affect ordinary transit-observations, such as errors in subdividing seconds of time, loss of time in mechanically registering impressions conveyed through the eye to the mind, &c. sink into insignificance compared with the three or four seconds which we had to allow for the probable error of a contact observation.

In observations of this kind the observers have to seize some well-marked phases of the gradual destruction of, and restoration of, the irradiation near the point of contact of the Sun's limb. The appearances are complicated by considerations of atmospheric disturbances, aperture of telescope employed, density of absorbing medium made use of to reduce the Sun's glare, eye of the observer, defining character of the telescope, and the power employed. But, under given conditions, the phenomenon — as, for instance, real contact or total disappearance of connecting ligament — will take place with a definite angular separation between the limbs.

If the attention is drawn to the first appearance of, or disappearance of, the connecting ligament, then that ligament must have a definite breadth, in order that, under the given circumstances of observation, it may be visible by contrast upon the Sun's disk. The question of seeing the ligament is not a question of sufficient time to enable us to pick it up, but of sufficient optical means to distinguish it. Such, at least, was and is my own opinion.

In the transit of *Mercury*, 1868, November, Mr. Carpenter, observing at Greenwich with a small telescope and low power, about 90, states that "the black ligament appeared to form *in-*

stantly, and to be of the breadth indicated, about $\frac{1}{3}$ the diameter of the planet." Now, although Mr. Carpenter observed the first formation, to him, of the black ligament, his time is about 13^s later than the time given by Mr. Lynn, observing with a power of about 170, and with a much better instrument. There was here plenty of time for Mr. Carpenter to see the ligament within the 13^s which elapsed between Mr. Lynn's observation and his own, but he did not see it, because, I maintain, his instrumental means were insufficient to appreciate the existence of the ligament by contrast, until it had become of a certain definite breadth. If this be the case, he would not have seen the ligament sensibly sooner had a period of 24^s elapsed whilst *Mercury* was moving over the distance from its position corresponding to Mr. Lynn's observations to that which it had at the time of his own observations.

The errors of such observations really arise when we assume that the phase of the phenomenon observed by one observer corresponds to the phase observed by another observer in such a way that each takes place at the same distance between the centres. The error thus committed is measured by the deviation of the true distance between the centres at which the observation was really made, and the mean distance for all the observations of apparently similar phenomena which are to be combined. The errors, therefore, measured in time, must be inversely proportional to the normal velocities. Another illustration, with a practical bearing, may be given, as follows:—Suppose that photography should be applied to the transit of *Venus*, by obtaining a great many photographs, by some quick process, whilst *Venus* is on and near the limbs, at points as near the points of maximum effect as possible. Then, with such a process, the errors in measuring the angular distances between the centres will be sensibly independent of the relative motions of *Venus* and the Sun; but if we should wish to apply these results to the determination of solar parallax by referring back all the photographs to some definite distance from the Sun's centre for comparison with photographs made at other stations, then, in referring these results back and determining the *time* at which *Venus* was at some definite distance from the Sun's centre, we should commit errors in *time* in proportion to the slowness of normal velocity.

These views led me to infer that the probable error of every single observation made in 1874 must be considered as injuriously affected by the slow normal velocity, and I gave in my April note the ratio of the mean probable errors arising from this cause in the transits of 1874 and 1882. I can understand differences of opinion existing with respect to the accuracy of these views; but I must confess my inability to find any full consideration of such views brought forward in the March number of the *Monthly Notices*.

On some Attempts to render the Luminous Prominences of the Sun visible without the use of the Spectroscope. By Warren De La Rue.

The progress of scientific discovery may be promoted by the record of one's failures, for it tends to prevent the same paths from being trodden by future explorers; on this account I venture to lay before the Society a description of some experiments on which I have been engaged since my return to Cranford in the spring of this year. Although these experiments have hitherto led to no satisfactory result, I am still pursuing them by varying the form of apparatus and the substances employed, in the hope of ultimately attaining the object in view. Notwithstanding the great success which has attended the study of the luminous prominences by Janssen, Lockyer, Huggins, Secchi, and more recently by Zöllner, it will be conceded that it is extremely desirable to find means of viewing these entities in their true position around, and on the Sun, without being compelled to distort the Sun's surface by spreading it out into an elongated sheet of light.

It must have undoubtedly suggested itself to many astronomers, that since the prominences emit light of but a few distinct and definite refrangibilities, it may be possible to absorb or to reflect out of the field of view the greater portion of all the other rays of the spectrum emanating from the Sun, by intercepting the light collected by an object-glass or mirror, by appropriate media. Indeed, the proposal of Huggins to combine the employment of the spectroscope with the use of absorbing media, naturally suggests the question,—why use the spectroscope at all?

The plan which I proposed to myself was twofold: to attack the F line on the one hand, and the C line on the other. In order to see the prominences by means of the F or green line, I adopted the following plan:—If the reader will suppose that in the case of a Newtonian, a right-angled prism is employed in lieu of the diagonal reflector, it will be evident that while the function of the hypothenuse is to reflect in totality the light it receives from the concave mirror, the two other sides of the prism are available for other objects, and may be covered by certain media through which the light must pass, and by which it may be altered in character by the reflection and dispersion into space of part of the rays, and the transmission to focus of others. The most promising covering medium for the right-angled surfaces of the prism appeared to me to be metallic gold, and fortunately the researches of Wernicke have furnished a means of obtaining a deposit of gold as brilliant and firmly adhering as the well-known silver deposits. The deposit of gold transmits a beautiful green, which includes the F line; and the Sun may be viewed without the slightest inconvenience after the light has passed through two films sufficiently thick. One advantage in gilding two surfaces of the prism consists in precluding the probability of any unaltered light from reaching the eye, for if minute holes occur in one film

it is not likely that others exist in the second film precisely opposite to them. The Sun's image when viewed through the gold films is beautifully defined, and perfectly cool, and very agreeable to the eye; indeed, if the hand be held in the principal focus of a 13-inch mirror, after the light has passed through the gold films, only a slight sensation of warmth is experienced; whereas a piece of wood, similarly placed, when the light is simply reflected by metallic mirrors or a prism not gilded, would soon be ignited: the gold film evidently, therefore, excludes most of the heat rays. Notwithstanding the hopes I entertained of rendering the prominences visible, I have not yet been able to succeed. There is, however, one circumstance which I ought to mention, because it may have contributed to the failure: it is this, it is extremely difficult to procure prisms in which the plane of the hypothenuse of the right-angled prism is truly parallel to the edge formed by the two sides at right angles. Very nearly true they are, but not perfectly so. In consequence of this imperfection, a great number of images of the Sun are seen (on account of a series of internal reflections), when the prism is held in the direction of the Sun; and although these may not overlap each other in the telescope, yet they cause an amount of diffused light which materially interferes with the possibility of seeing the prominences. The most obvious remedy is, of course, to try to procure a true prism; but the Astronomer Royal, to whom I imparted my difficulty, has suggested passing the light through pairs of prisms of small angle, superposed so as to form an optical monad, which would act as a parallel plate of glass, but in which the component prisms offer four sides which may be gilded. If one monad is not sufficient to screen away all the useless rays, then a second or a third might be used.

It may be mentioned that, in the case of the reflector, I have used a second smaller right-angled prism with two faces gilded, and placed between the eye-piece and the first prism before mentioned, and that the Sun's image was still very bright after passing through four films of gold. Also that I have employed a gilt prism with my 4-inch Dallmeyer.

It is not impossible that I may succeed in obtaining a photographic image of the prominences by using the gilt prism, even though I may not succeed in seeing them. Before, however, abandoning the attempt of rendering them visible by means of the gold films, I am having made sets of prisms, as proposed by Mr. Airy, and shall also endeavour to obtain true right-angled prisms; unfortunately, the Sun's low altitude will interfere for some months with the prosecution of the experiments.

Now, with regard to the C or red line, my plan of attack has been to employ red fluids, placed either between the eye-piece and mirror or object-glass, as the case may be, or between the eye-piece and eye. The two as yet experimented on without result are, a solution of carmine in ammonia, and a solution of aniline red in alcohol.

It is, of course, possible to ascertain beforehand whether the C ray is transmitted by the fluid, provided a suitable spectroscope is used; a trial showed that the red light transmitted by a solution of carmine in ammonia included the F ray, but this solution did not bring to view the luminous prominences. I am now preparing some pure carminic acid and some compounds of that acid for future experiments, and I am having a series of bottles made of larger capacity than those I have hitherto employed; for I have found that unless the light passes through one or two inches, or even more, of some of the coloured fluids, the eye is inconvenienced by the great brilliancy of the Sun's image. The vessels used have, of course, two sides of parallel glass.

On the Floor of Plato. By W. R. Birt.

During the last forty-eight years, occasional notices of the spots and marking on the floor of the walled plain *Plato* have appeared. In consequence of having given considerable attention to *Plato* and its surroundings in the years 1860 to 1863, I collected all the observations of the spots that I became acquainted with, numbering in the whole fifty-six, having reference to fourteen spots, two or three of which had been observed as craters, one being double. The greatest number observed *simultaneously* was seven, by Gruithuisen, in the year 1825. On the 23d of February of the present year, Mr. Pratt, of Brighton, observed with his 8-inch silvered-glass reflector eleven spots at the same time. They were not, however, eleven of the spots which had been observed previously, but included four unrecorded and a second double spot. Mr. Pratt has steadily continued his observations up to the present time, and determined the relative positions of twelve spots by alignment. Mr. Edward Crossley, of Halifax, has also kindly requested his assistant, Mr. Joseph Gledhill, to make continuous observations on them with his 9·3-inch achromatic by Cooke. The observations made by Mr. Gledhill and Mr. Pratt I have regularly received. The following are the names of the astronomers who have observed the floor of *Plato*. Gruithuisen, Mädler, Challis, Knott, the late Lord Rosse, the late Rev. W. R. Dawes, Baxendell, Dr. Dobie, Birt, Pratt, Crossley, Gledhill, and Elger, the number of observations being 297, in fifty series, and the number of spots observed twenty-five, including the companion of Dawes's double spot. In the annexed Table the number of each spot is given in accordance with the accompanying diagram, the name of the discoverer, the number of times each spot has been observed since the commencement of the present year, 1869, the comparative degree of visibility, that of the central spot, No. 1, being reckoned as unity, or 1·00, and remarks on the positions and general characters of the spots.

Spots on Plato.

No.	Discovered by	Obs.	Visibility.	Remarks.
0	Gruithuisen			N.W. of 1, apparently nearer than 23.
1	Gruithuisen	32	1·000	Central, easy, generally of the same appearance.
2	Gruithuisen	1		Near the W. rim, seen once only by Gledhill.
3	Gruithuisen	23	·782	Dawes's double crater, frequently seen single.
4	Gruithuisen	27	·844	Departs often from typical state; seen double by Pratt.
5	Challis	20	·625	Not seen so frequently as 3.
6	Gruithuisen	8	·250	Seen lately by Pratt and Gledhill.
7	Gruithuisen	8	·250	Seen lately by Gledhill, Pratt, and Elger.
8	Gruithuisen			Probably the S.E. companion of 3.
9	Dobie	5	·156	A minute spot S.E. of 1, seen by Dawes and Gledhill.
10	Pratt	6	·187	Seen by Gledhill and Elger.
11		1		A minute spot between 9 and 17, seen once by Gledhill.
12	Gledhill	3	·094	N. of No. 14; seen only by Gledhill.
13	Pratt	17	·531	On curved streak c, now frequently seen.
14	Pratt	21	·656	On central arm of "trident," frequently seen.
15	Dawes	1		A minute spot due E. of 1, seen once by Gledhill.
16	Pratt	13	·406	On curved streak c, frequently seen.
17	Mädler	21	·656	On wedge of "sector," frequently visible.
18	Gledhill	1		Near N.W. arm of "trident," alignment 1 : 18 : 22.
19	Gledhill	17	·531	On curved streak c, frequently seen.
20	Knott	3	·094	Near N. border, probably seen by Gledhill.
21	Knott			E. of 20, near N. border.
22	Mädler	9	·281	Seen lately by Gledhill, Pratt, and Elger.
23	Pratt	1		Alignment 19 : 23 : 4.

As the comparative visibility of a spot depends upon its size and brightness, the larger and brighter spots are more likely to be seen in indifferent states of the Earth's atmosphere than those which are small and of less brilliancy. The number of observations, 238, between Feb. 23 and Sept. 27, 1869, afford a preliminary basis for an approximate measure of the degree of visibility of each according to the number of times it has been observed. The present order of visibility is as under :—

No.	Visibility.	No.	Visibility.	No.	Visibility.	No.	Visibility.
1	1·000	17	·656	16	·406	10	·187
4	·844	5	·625	22	·281	9	·156
3	·782	13	·531	6	·250	12	·094
14	·656	19	·531	7	·250	20	·094

In most cases the degree of visibility is underrated, as it is probable that in another series by the same observers many of the smaller spots may be *looked* for and *seen* under circumstances which, in earlier observations, occasioned them to have been overlooked, unless specially sought for. The spots remaining in the same condition, affected only by variations in the Earth's atmosphere, the probability is that successive series of about the same number of observations by the same observers will give similar degrees of visibility. It may be remarked that on no occasion have all the spots been seen simultaneously; the greatest number recorded in one watch is thirteen, and these were not all seen at once.

The dotted lines in the diagram are intended to represent the probable boundaries of the lighter markings on the floor of *Plato,* which are by no means of a sharp or definite character, but ill-defined and passing gradually into the darker hue. Although there is some reason to conclude that they are really permanent, yet they are seldom seen alike. The observations of these delicate markings are not at present sufficiently numerous to allow of an examination into the circumstances that affect their visibility. In conclusion, I would remark that this communication is intended rather as a contribution towards elucidating a confessedly difficult branch of Selenography, than establishing the order of visibility of the spots above named, further observations being required for this object.

Lunar Eclipse of 23rd and 24th *July,* 1869. By John Tebbutt, Jun.

The lunar eclipse of the 23–24th July was observed here as follows:—

	Windsor M.T. d h m s
First contact with shadow	23 10 43 35
Last ,, ,, ...	13 29 28

The contacts were observed with a power of about 90 on my 3¼-inch refractor, but the results are by no means satisfactory, owing to the irregular and ill-defined character of the shadow.

The colour of the shadow was very dark iron grey; the red tint usually seen in total eclipses was not noticed. Even with a power of about 30 and the illuminated disk excluded from the field of view, the details on the obscured portion of the surface were perceived only with the greatest difficulty. The eclipsed limb was, however, pretty distinct, but it appeared to be an arc of a smaller circle than the illuminated limb, the bending inwards of the limb at the cusps being very perceptible.

Windsor, New South Wales, August 11, 1869.

Comet 1869.

Discovered October 11, at Vienna, by M. Tempel.

Mr. Hind, in a letter dated Twickenham, Nov. 17, addressed to the President, writes, that Dr. Winnecke having sent him an observation on October 18, which fits in very well with two in the *Astronomische Nachrichten,* he has calculated the following approximation (apparently a very fair one) to the elements of the orbit:—

Perihelion Passage	1869, Oct. 8·4421 G.M.T.	
Longitude of Perihelion	$124^{\circ}\ 41'{\cdot}1$	Apparent Equinox.
,, Ascending Node ..	$311\ \ 24{\cdot}4$	
Inclination	$68\ \ 48{\cdot}8$	
Log. perihelion distance	0·0895	

Motion retrograde.

He remarks that these elements do not resemble those of any comet previously computed. They appeared in "*Nature,*" No. II.

Occultations of Stars, and Eclipse of Satellite of Jupiter, observed at Leyton. By C. G. Talmage.

1869, July 21.—Occultation, disappearance of 33 *Sagittarii;* power 70 on 10-inch refractor. G.M.T. $12^{h}\ 3^{m}\ 23^{s}{\cdot}92$.

Good, time exact, cloudy at reappearance.

1869, August 2.—Occultation, reappearance of α *Tauri;* power 70 on 10-inch. G.M.T. $13^{h}\ 13^{m}\ 48^{s}{\cdot}23$.

Came out instantaneously, no hanging on limb. Moon hid by a tree at disappearance.

1869, October 11.—Occultation, reappearance of 31 *Sagittarii* on Moon's bright limb; power 70 on 10-inch. G.M.T. $6^{h}\ 7^{m}\ 55^{s}{\cdot}93$.

It was too light for disappearance, as light clouds covered the Moon.

1869, October 11.—Eclipse, disappearance of *Jupiter's* II. Satellite. G.M.T. $8^h\ 50^m\ 37^s{\cdot}21$.

Excellent definition, time good.

Mr. Barclay's Observatory, Leyton, Essex,
Nov. 11, 1869.

Instrument for Sale.

A 30-inch Transit Instrument by Troughton and Simms, for mounting on stone; $2\frac{1}{2}$ in. object-glass. Declination circle, divided on silver, with two verniers, reading to $\frac{1}{2}$ min. Three eye-pieces, 37, 66, and 100. Diagonal prism. Micrometer to eye-piece, with Level, complete in case. Price 35*l.*; cost 55*l.* Apply to L. P. Casella, 23 Hatton Garden, London, E. C.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *December* 10, 1869. No. 2.

ADMIRAL MANNERS, President, in the Chair.

Charles Joseph Corbett, Esq., Imber Court, Thames Ditton;
Charles Lambert, Esq., Queen Street Place, Thames Street;
and
John Wood, Esq., Wharf College, near Tadcaster,

were balloted for and duly elected Fellows of the Society.

The November Meteors, 1869, observed at Port Saïd. By G. L. Tupman, Esq.

On the morning of the 8th November, while observing shooting stars, I suspected radiation from the constellation of *Leo*. The following night was too cloudy for any observations whatever to be made.

On the 9th, between $12^h\ 45^m$ and $15^h\ 45^m$, radiation from *Leo* was observed at the rate of 8 per hour, principally small meteors. At the same time the radiation from two radiants in *Taurus* was at the rate of 12 per hour.

On the 10th, between 13^h and 16^h, the radiation from *Leo* was at the rate of about 6 per hour, and from *Taurus* about 7 per hour.

On the 11th, from $12^h\ 50^m$ to $15^h\ 10^m$, there was no radiation whatever from *Leo*, or indeed from any other point, although during that time shooting stars were observed at the rate of 16 per hour.

I am, as yet, unable to determine the radiant point in *Leo*, on the 9th and 10th, so that it is impossible to say if the meteors mentioned belonged to the true November system or not. Probably not.

On the 12th, out of thirteen observed meteors, four certainly radiated from near γ *Leonis*. One at $14^h\ 50^m$ passed exactly over the stars μ and α *Ursæ Majoris*, leaving a dazzling streak midway between those stars. This number reduced to the zenith would give about 5 per hour for a single observer.

On the 13th, a very strong breeze was blowing from the northward, accompanied by heavy clouds, which occasionally nearly altogether obscured the heavens. Above the lower cloud stratum was a thick haze which prevented the smaller stars being seen.

From $12^h\ 30^m$ to $13^h\ 15^m$, a pretty large patch being clear overhead, two meteors only were seen, neither of which radiated from *Leo*. It then became overcast, and I felt convinced that the shower was either all over or had not yet commenced.

The watch was resumed at $14^h\ 30^m$, the sky being then partly clear in patches, and continued until a quarter past 5, long before which the shower had entirely ceased. At $2^h\ 30^m$ it was at its height, most of the meteors being remarkably brilliant, and many of them tinted green. The greater part left bright streaks, which often remained visible a considerable time. The duration of the meteors or their "time of flight" was considered to be less than half a second—too short a time to estimate even roughly.

The following are the observations. Being unassisted, I stopped at every sixteenth to make the necessary entries:—

13 November, 1869, Alexandria Mean Time.

From	To	No. of Meteors.	Elevation of the Radiant.
h m	h m		°
10 40·0	13 15·0	0	15
14 30·0	14 40·0	16	35
14 52·0	15 2·5	16	40
15 8·0	15 19·7	16	43
15 24·0	15 33·6	16	46
15 38·5	15 52·5	16	50
15 59·0	16 7·4	16	54
16 12·0	16 24·0	16	57
16 26·0	16 38·0	6*	60
16 40·0	16 52·0	7	63
16 54·0	17 14·0	4	67

Seven other meteors were observed, but they did not radiate from *Leo*.

If the numbers in the above table be reduced to an uniform

* During this observation it was more cloudy than before, but during the two following ones it was much clearer.

interval of time and then multiplied by the cosecant of the altitude of the radiant, it will be seen that between $14^h\,30^m$ and $16^h\,24^m$ the numbers were nearly uniform, and slightly decreasing. The maximum, then, was either before or about $14^h 30^m$; but the *centre* of the dense part must have been passed about 15 hours, as there was no sign of the shower at $13^h\,15^m$.

For the determination of the Radiant point, eleven orbits were marked off on the chart, which appeared so close to the radiant as to give its position much more accurately than a host of orbits further off. The following table is a list of these orbits. The right ascensions and declinations are measured from the equinox of 1830, and, consequently, the resulting radiant point requires correcting for 39 years precession. Those of the first order of merit are more valuable than the others :

No.	Appearance. α	δ	Disappearance. α	δ	Order of Merit.
	°	°	°	°	
1	151½	+30	152	+42	2
2	154½	+21¼	158	+21¼	1
3	161	+20¾	172	+19¾	2
4	152½	+19¼	157	+16¾	2
5	150	+19½	149¼	+14	1
6	149¼	+19	147½	+14½	1
7	140	0	144	−12	2
8	130	+ 7	117	− 3	2
9	112	+ 5½	84	− 8½	1
10	143	+19½	136	+16	2
11	140	+19½	127	+17½	2

These give the Radiant $\alpha = 151^\circ{\cdot}0$, $\delta = 21^\circ{\cdot}5$, measured from the equinox of 1869. But it must be noticed that no single point will satisfy all the paths that were observed, which proves that their orbits cannot be identical in inclination and eccentricity.

Three of the meteors deserve special notice.

The first occurred at $15^h\,32^m$ near θ *Hydræ.* After describing an arc of 10° or 12°, almost instantaneously it exploded with a blaze of light that illuminated the whole sky, and was mistaken for lightning. At the point of explosion, a small luminous cloud was formed, about a degree and a half in diameter, of the shape of a nearly closed horseshoe, the interior diameter being about one-third the exterior. Its whole light was about equal to that of a first magnitude star, or of the nebula in *Lyræ*, seen in a large telescope. During the two or three minutes that attention was paid to it, it did not sensibly diminish its lustre or alter its place, which was $\alpha = 141^\circ$, $\delta = + 4^\circ$.

The second occurred at $15^h\,40^m$ in *Ursa Major.* It lighted up everything around as a brilliant flash of green lightning, and left an exceedingly bright streak some 8° in length, the centre of

which was in $\alpha = 131°$, $\delta = +55°$. After about a minute, this streak assumed a beautiful wavy appearance ~~~~, and two or three minutes after one of the shape of an S, the axis of which was inclined to the original axis about 70°. Five minutes after its first appearance it had drifted over the star o *Ursæ*, and was still pretty bright when it was obscured by clouds.

The third was very bright, of a greenish hue, and exploded like the first, but behind thin cloud, which did not prevent its illuminating all around. Unfortunately, its position could not afterwards be identified.

On the 14th, from $14^h\,30^m$ to $15^h\,20^m$, about a third of the sky being very clear, five small shooting stars only were seen, radiating from *Orion*.

The thickness of the dense part of the stream must have been about 52,000 miles, measured perpendicularly to the plane of the orbit. It may only have been one aggregation among many, and may not have been situated centrally in the stream.

From these observations the elements of the orbit are

$$\begin{aligned} \pi &= 62°\ 36' \\ \iota &= 15\ \ 38 \\ \Omega &= 231\ \ 44 \\ \epsilon &= \cdot 9062 \end{aligned}$$

Motion retrograde.

Assuming a periodic time of $33\frac{1}{4}$ years.

Port Saïd, Lower Egypt,
16 *November*, 1869.

The Assistant-Secretary, Mr. Williams, read some extracts from the introductory remarks to the MS. work presented to the Society by him at the last meeting, being a translation of the accounts of comets observed in China from B.C. 613 to A.D. 1640, with the original text, of which introductory remarks the following is a brief abstract:—

According to Chinese tradition the Emperor Shin Nung, the successor of Fuh He, the founder of the empire, was the first who instituted astronomical observations. His reign commenced B.C. 3218. One of his successors, Hwang Te, is reputed to have been the monarch who established the mode of reckoning their chronology by cycles of 60 years, in use to the present day. The first of these cycles commenced B.C. 2637. He is also said to have discovered the lunar cycle of 19 years, by which the intercalary moons were to be regulated, from which it should appear that this cycle was known to the Chinese about 2000 years before it was introduced into Greek astronomy by Meton. These, however, must be looked upon merely as Chinese traditions, having no other authority.

In the most ancient of the Chinese historical works, the Shoo King, the instructions of the Emperor Yaou, who ascended the throne B.C. 2356, to his astronomers He and Ho are mentioned. In these they are, first, commanded to record their observations of the heavenly bodies, to complete the calendar, to make an instrument to show the motions of the Sun, Moon, and stars, and with due respect to impart information respecting the seasons to the people. In the succeeding four paragraphs four astronomers are sent to places in the east, south, west, and north, to observe the equinoxes and solstices, and certain stars are named as indicating the seasons. The first of these stars answers to our *α Hydræ*, which is described as culminating at sunset on the day of the vernal equinox. From this it seems that the vernal equinoctial point was then in the *Pleiades*, and consequently by the precession of the equinoxes it would appear that this observation was made B.C. 2306, being the 56th year of the reign of Yaou, thus affording a strong presumptive proof of the veracity of Chinese history as recorded in the Shoo King. The remaining three paragraphs relate to the labours of the other three observers; and, in conclusion, Yaou calls the attention of the astronomers to the fact that the year consists of 366 days, and among other matters of less importance to astronomy directs them to fix the intercalary moons, by which the seasons were to be kept in their places. Many other works are mentioned, both ancient and modern, as containing astronomical notices, and among these the great historical work called the *She Ke* and the Encyclopædia of Ma Twan Lin, are more particularly noticed as being those from which the greater number of the observations of comets contained in this volume have been extracted. The number of these observations thus brought together amounts to 370, a few of these may be meteors, but the far greater number are undoubtedly comets. Those mentioned by E. Biot in his communication to the *Connaissance des Temps* for 1846, amount to 224, consequently, in the present catalogue there are about 146 more than are given in that work; and there is reason to believe that the list now under consideration is as complete as any that has hitherto appeared.

The observations taken singly may be considered as divided into two parts, the one chronological, and the other astronomical. For reducing the first to our reckoning, certain tables have been constructed consisting of a complete set of chronological tables to be used in finding the years, and of others needed in reducing the moons and days. The construction and use of these tables are fully explained, and illustrated by examples of their application, and by their aid any year, moon, or day mentioned in the Chinese records can readily be reduced to our time.

For the astronomical portion, a complete celestial atlas, traced from the cuts in a Chinese work on astronomy, has been made, and which, as well as the preceding tables, forms a part of the work. In this the twenty-eight stellar divisions and the three

great spaces in which the stars are distributed by the Chinese are figured and described. To this atlas there is a full explanation, so that any group of stars mentioned in Chinese astronomy may be readily found by means of the index to the names; and the accordance of these groups with our constellations, is also made clear by means of reduced copies of the figures in Flamsteed's Atlas, with the Chinese asterisms laid down on the corresponding stars. There are many other interesting particulars mentioned in these introductory remarks which time did not allow to be entered upon, particularly an enumeration of the subjects treated of in the sections and chapters of the astronomical division of the history of the Ming dynasty 1368–1644, which, among other matters of interest, contain not only cometary observations, but also a catalogue of stars, with their longitudes and latitudes, both on the equator and the ecliptic, and more than seventy pages of observations of occultations of stars by the Moon and the planets.

Mr. Williams concluded by expressing his conviction that in placing this volume in the library of the Society he had secured it a position in which it is the most likely to be of service in future investigations into the subject of Chinese astronomy.

On Auroral Appearances and their Connexion with the Phenomena of Terrestrial Magnetism. By Balfour Stewart, F.R.A.S.

Some years since I ventured to suggest that auroral displays might be secondary currents due to small but rapid changes, caused by some unknown influence in the magnetism of the Earth. In developing this idea, the Earth was compared to the core of a Ruhmkorff machine, and the moist upper strata of the Earth, as well as the upper strata of the atmosphere, to secondary conductors, in which currents will take place whenever the magnetism of the Earth changes from any cause. These views would appear to be confirmed by the very interesting records of earth-currents obtained by Mr. Airy at the Greenwich Observatory, in which it is found that during times of great magnetic disturbance there are strong earth-currents alternating from positive to negative, the curves lying nearly equally on both sides of the zero.

A further development of this idea has lately occurred to me, in consequence of a remark of my friend Mr. Lockyer, that the zodiacal light may possibly be a terrestrial phenomenon, and may therefore be somehow connected with the phenomena of terrestrial magnetism. For not only will secondary currents be caused in a stationary conductor in presence of a magnetic core of variable power, but also in a conductor moving across the lines of force of a constant magnet. The question arises, have we on the Earth such moving conductors? In answer to this, let us reflect what takes place at the equator. When once the anti-trades have reached the upper regions of the atmosphere,

they will become conductors from their tenuity; and as they pass rapidly over the lines of the Earth's magnetic force we may expect them to be the vehicles of an electric current, and possibly to be lit up as attenuated gases are when they conduct electricity. May not these form the Zodiacal light?

Such moving currents will of course re-act on the magnetism of the Earth. We may therefore suppose that somewhat sudden and violent changes are likely to take place in the Earth's magnetism at those seasons at which the Earth's great wind-currents change most rapidly. May not this account for the excess of disturbances at the equinoxes?

Besides the anti-trades there are also no doubt convection currents caused by the daily progress of the Sun taking place in the upper regions of the Earth's atmosphere. May not these also be the vehicle of currents as they cross the lines of the Earth's force, and account, to some extent at least, for the daily variations of terrestrial magnetism? and may not this be the reason of the likeness observed by Mr. Baxendell between the curves denoting the daily progress of the wind and those denoting the variation of the declination magnet? Such currents, in as far as they are electric conductors, taking place in the upper regions of the atmosphere would not be felt by the earth-current wires at Greenwich, and I think Mr. Airy has noticed that this is the case. But the tidal wave represents a motion of a conductor on the Earth's surface, with two periods in one lunar day. This motion cannot produce a very great secondary current, but may it not be sufficient to account for the lunar-diurnal magnetic variation, which is also very small?

Such a current taking place in a conductor electrically connected with the Earth's upper surface ought to be felt by the Greenwich wires, and, if I am not mistaken, Mr. Airy has detected a current of this nature.

May we not also imagine that there are two varieties of Aurora, one corresponding to stationary conductors under a very rapidly changing core, and the other to rapidly moving conductors under a constant core? And might not an Aurora of the latter kind indicate the approach of a change of weather?

These remarks are thrown out in order to invite comment and criticism, and they will have served their purpose if they direct attention to the part that may be played by moving conductors in the phenomena of terrestrial magnetism. It will be noticed that these remarks do not touch upon the mysterious and interesting connexion believed to exist between magnetic disturbances and the frequency of solar spots.

P.S.—Since writing the above, Sir W. Thomson has called my attention to a paper by him in the *Philosophical Magazine* for December 1851, in which it is suggested that moving conductors may play a part in the phenomena of terrestrial magnetism.

Note on Mr. De La Rue's paper "On some attempts to render the luminous prominences visible without the use of the Spectroscope." By William Huggins, Esq.

Mr. De La Rue puts the following question, "The proposal of Huggins to combine the employment of the spectroscope with the use of absorbing media naturally suggests the question,—why use the spectroscope at all ?"

The suggestion contained in this question was one of the methods by which I originally attempted to render the red flames visible. Some three or four years since, almost at the same time that the method of reducing the scattered light of our atmosphere by the use of dispersion occurred to me, the method by absorption also presented itself to me as a very feasible plan. I made many attempts, not only with the spectroscope, but also with variously combined coloured media without the spectroscope. The latter plan, namely, by absorption, appeared to me to be peculiarly desirable, if it could be accomplished, as it would enable the observer to see the forms of the prominences, and also the whole disk of the Sun at once. It could be applied to any telescope, or used with an opera-glass, or even with the naked eye.

I referred to this method in the report of my Observatory, *Monthly Notices*, vol. xxviii. p. 88. Subsequently, when the Indian observations had confirmed my suspicion that the prominences would give bright lines, and also shown their position in the spectrum, I gave in a short Note an account of this possible method of viewing the solar prominences: (*Monthly Notices*, vol. xxix. p. 4.) The method is also described in my paper in the *Phil. Trans.* 1868, p. 551.

This method was also independently suggested by Lieut. John Herschel in September, 1868: (*Monthly Notices*, vol. xxix. p. 5.)

During the early part of the present year I tried a large number of coloured media. The difficulty is to combine two media which shall absorb light of all refrangibilities except precisely that of the line C or the line F. If even a small range of refrangibility besides that of the line selected, say C, be allowed to pass, the scattered light of the atmosphere overpowers and eclipses the prominences.

The most promising media of those which I tried, were a solution of carmine in ammonia, which cuts off very nearly all the light more refrangible than C, combined with a solution of chlorophyll, which gives a strong band of absorption, taking away the brighter part of the light less refrangible than C. Unfortunately, however, the chlorophyll band encroaches a little upon C, and so weakens the light of the prominences. The absorption of chlorophyll, as Prof. Stokes has shown, can be moved a little in the spectrum by acids and alkalies, and differs slightly in different plants; but I have not been able to degrade the band sufficiently to allow light of the refrangibility of C to pass wholly unimpeded.

The proposal stated by Mr. De La Rue of combining the employment of the spectroscope with the use of absorbing media, refers probably to a method I proposed for rendering the *forms* of the prominences visible in the spectroscope. In a Note to the Royal Society, *Proceedings*, vol. xvii. p. 302, I showed that by using a wide slit, the forms of the prominences could be seen directly; and I then proposed using a deep red glass before the eye-piece of the spectroscope for the purpose of relieving the eye from the glare, which, under some circumstances, is painful, when a wide slit is employed.

On Spectroscopic Observations of the Transit of Venus in 1874. By R. A. Proctor, B.A.

At the last Meeting of this Society it was mentioned that the American observers had suggested that spectroscopy should be applied to the observation of the approaching transits of *Venus*. It had been noticed during the total eclipse of last August that the approach of the Moon to the Sun's disk was rendered sensible before the actual commencement of the eclipse by the obliteration of the bright lines of the chromosphere; and the American astronomers suggest that the same might be the case when *Venus* is approaching the Sun's disk in 1874.

I think Mr. Huggins gave a practical character to this suggestion by pointing out that, although there would be difficulties in the method, applied merely to determine the motion of *Venus* over the chromosphere, yet that, as giving a means of preparing the observer for the occurrence of external contact, the method would perhaps be valuable. It is clear that this is, in fact, the only sort of observation which would have any value, since the chromosphere is not an envelope of uniform depth, and *Venus*, as seen from different parts of the Earth's surface, would appear to cross different parts of the chromosphere.

There is a further objection which I pointed out at the last Meeting in the fact that *Venus* approaches the Sun's limb not at a right, but at an exceedingly acute, angle, and that, as we do not know certainly the point of contact, we cannot tell the angle at which *Venus* will cross the limb.

Further, the stations best suited for observing internal contacts are not all suited for the particular mode of observation proposed by the American astronomers. At Crozet Island, for example, where internal contact most retarded can be most favourably seen, the Sun will be close to the horizon when *Venus* is traversing the chromosphere.

Passing over this last objection, which applies equally to egress and to ingress, it will be clear that Mr. Huggins' excellent suggestion that the passage of *Venus* over the chromosphere can be made the means of determining with extreme exactitude, the moment of *external contact*, can be applied satisfactorily to ob-

servations of *Venus* in egress, since the observer can follow the planet across the Sun's limb with perfect certainty as to the point of last contact.

It has occurred to me that the method of observing the prominences by means of an open slit (high dispersive power being used) might be adopted with advantage for determining the moments not only of external, but of internal contacts.

It is clear that so far as external contacts are concerned, any method which rendered the prominences and the chromosphere visible would serve to exhibit the passage of *Venus* over these objects, and so to indicate with extreme exactitude her first contact with the Sun's limb. I would invite particular attention to the extreme slowness with which, in 1874, *Venus* crosses the limb of the Sun. In a paper which appeared in the Supplementary Number of the *Monthly Notices*, I called attention to certain effects of this peculiarity. According to ordinary modes of observation these effects would be disadvantageous, though they are compensated by other advantages. But, according to the suggested mode of observation, the slowness with which *Venus* approaches the Sun's limb would be a great advantage. If we assign to the chromosphere an apparent height of about 10″, *Venus* will occupy about $4\frac{1}{2}$ minutes in crossing that envelope, and this would afford the observer ample time to search for her. At egress, of course, it would be quite easy to follow the motion of her disk across the chromosphere.

In this plan the direction of the open slit would be at right angles to the Sun's limb; and it will be very obvious that internal contacts could be watched in the same way, and, perhaps, with more exactitude than by the ordinary method.

But it has occurred to me that internal contacts might be observed with advantage in a different manner, and probably Mr. Huggins had some similar plan in his thoughts, when at the last Meeting he suggested the use of a tangential slit.

If the open slit were placed tangentially, so as to include the solar cusps *a* and *b*, it is very clear that there would be seen two solar spectra separated by a dark space across which could lie the image of the chromosphere, which, however, would not be visible, owing to the brightness of the solar spectra. As *Venus* gradually moved from the Sun's limb the two solar spectra, or at least their inner edges, would appear to approach, and at the moment of real internal contact the dark space between them would disappear.

I think the moment of internal contact might thus be determined much more exactly than by direct observation. Of course, the method in reality amounts to changing the points of the cusps into parallel

lines and determining the moment at which these lines become coincident. But it seems obvious that, so far at least as optical difficulties are concerned, an observation of this sort would be much more easily made than an observation of "the breaking or formation of the dark ligament."

This method is clearly applicable to egress also, and applies as well to Halley's as to Delisle's method. The best stations for applying it are those referred to in my paper in the *Monthly Notices* for last June.

Nov. 22, 1869.

On a Change in the Colour of the Equatorial Belt of Jupiter. By John Browning.

For several years I have been in the habit of observing *Jupiter* with considerable regularity with reflectors of large aperture. On every occasion previous to this presentation, the equatorial cloud-belt has been without colour, and the brightest portion of the disk of the planet. During the month of October this cloud-belt has, however, constantly been of a strong, greenish yellow, and darker than the bright belts north and south of it. The colour is almost exactly that known to artists as yellow lake. The colours of various parts of the disk I take from my observatory note-book.

Oct. 9th, 9·30.—Equatorial belt strong yellow lake, the other bright belts colourless. Dark belts very warm grey, and faint coppery red. The poles of the planet ashy blue. The drawing I have the honour of exhibiting represents the planet as it appeared at this time.

Two or three persons quite unused to the telescope noticed the colour of the equatorial belt.

Oct. 11th.—Observed *Jupiter* with Mr. Proctor. Colours the same, but the greenish-yellow belt was covered with white spots. Mr. Proctor agreed with me as to the greenish yellow of the equatorial belt and the ashy blue of the poles, but could not see the red in the dark belts.

In all instances I used 12 inches aperture and a power of 148, *achromatic* eye-piece.

I have received a letter from Mr. Brindley, of Lewisham, from which I give an extract:—"With reference to the appearance of *Jupiter*, I last night spent some hours with the 8½-inch telescope turned on him. I was most agreeably surprised; the belts were so different to what I had seen them before. The dark ones of a dark lake colour, the bright one of a lovely tinted green." As this letter was written some days after my drawing was made, and the writer had not seen the drawing, I cannot be much mistaken in the colours I have ascribed to the various portions of the planet.

Two years since I saw a coloured drawing of *Jupiter* at Mr. De La Rue's Observatory at Cranford. In this drawing, if my memory serves me, the equatorial belt was colourless.

I have watched the planet through the greater part of a revolution; the greenish yellow of the great equatorial belt remained unchanged. I have drawn the attention of several observers to the colours of the belt, some of whom have been in the habit of observing it for many years. One of these gentlemen says that he is certain that the equatorial belt has not presented such an appearance as at present for a quarter of a century.*

Writing to Mr. Proctor some time since, I said that I thought the very small specific gravity of *Jupiter* might be due to the existence of a cloudy envelope of enormous extent. Mr. Proctor replied, that he thought it just possible that the whole light from the planet might not be reflected; but that some portion might be emitted by the body of the planet. The same idea had presented itself to me. Being by far the largest planet in the solar system, it will certainly have retained more heat than the rest.

Such an alteration in the colour of the planet as I have described, must, I think, indicate some considerable change, either on the surface of the planet or in its atmosphere. In the hope of throwing some light on such changes in future, I am making a careful map of the spectrum of the planet. When this is completed I shall have the honour of submitting it to the Society.

Selenographical Notes: Apenninus and adjacent Regions. By C. H. Weston, Esq.

Few lunar objects are more striking than the range of *Apenninus* lying S.E. of *Mare Imbrium*. It not only presents forms unlike the commonly existing circular structures, but when viewed on the *terminator* of an increasing Moon (and definition good) exhibits broad shadings on its massive flanks and serrated outlines mapped out in light upon the low lands far beneath.†

During the last month the decreasing Moon was examined under favourable circumstances on the morning of the 27th when just east of the meridian. Her age about $20^d\ 13^h\ 30^m$. By her libration a little more of the NNE. quadrant was made visible than in mean position. The *terminator* was particularly interesting, as its extremities passed so very near both the lunar poles. On or near the *terminator* the following striking points were carefully identified:—Region near North Pole, *Anaxagoras* on east of terminator, *Barrow A* on west (shadow fine), *Epigenes* on east, *Aristoteles*, *Eudoxus*, *Calippus*, on east, *Menelaus* on east, *Agrippa* on east, *Godin* on east, *Delambre*, *Taylor* on east, south-west end of valley between *Taylor* and b.c. south-east of *Taylor*, *Kant*, *Descartes*, *Abulfeda*, *Almanon*, *Sacro-bosco*,

* Since writing the above I have received information which leads me to suspect periodicity in the change of colour on *Jupiter*.

† During a partial eclipse (when illumined objects could be contrasted with dark parts) details were finely seen by the larger Newtonian (14·25 in. 16 ft.)

Gemma Frisius, Maurolycus, Clairaut, Jacobi, Pentland, Simpelius, and region north-west of South Pole. On the south-east *Sharp* could be detected, and the high mountain range north-west of *Newton.** The extreme south peripheral regions of mean Moon were cut off by libration.†

On 26th *Scoresby* (south-west of North Pole) and two contiguous *ellipses* were seen, but (by the same libration) under greater than the mean visual angles, and therefore apparently more circular than in Beer and Mädler's map. *Gioja*, too, and the mountainous range still further north, which conceals the North Pole point.‡

It is best, first, to describe the cliff-like coast line of *Apenninus* and adjacent regions areographically. The lower general surface of *Mare Imbrium* (except when broken by the rays of *Copernicus* and low ranges) is apparently continued at about the same level to *Apenninus*, and extends from *Eratosthenes* to the vicinity of *Huygens*, where, I conceive, should have been introduced in the valuable lunar map an equally dark continuous shading, as far as and around *Huygens*, to meet the shading at the foot of the promontory in the rear, and so sweeping round and onward to the ranges still more retired. At the last point begins the elevation of the floor of *Mare Imbrium*, and extends north-east towards *Timocharis* and round to *Archimedes*. I do not think, however, that this elevation should have been continued unbroken or so little broken. There seems to be an interrupted valley of some width, running north-east and south-west from *Archimedes* to *Apenninus*, co-extensive with the base of the former, and somewhat, perhaps, wider near the latter. Under low telescopic powers (in this phase of the Moon) it assumes not only a tripartite structure, but (at the end abutting against *Apenninus*) a broken shaded triangular form. Under high powers we perceive in it incipient or low protrusions, circular and rugged, and the shaded triangular part loses its character and is found marked with several hill ranges and deep dark combes. The *south-west patch* has considerable ridges, consisting apparently of great arcs of curves, connected like festoons with the convexities towards the valley, and together occupying about its whole length. It is true, indeed, that at full Moon the valley shows the brilliancy characteristic of raised lunar surfaces, but the experienced eye can see that while above the level of the *Mare Imbrium* it is lower than the contiguous patches.§

* A peak in the vicinity is the highest part of the Moon ("Der Mond," B. & M.)

† Under favourable libration and latitude the exact locality of South Pole can be observed through an opening in *Malapert* (B. & M.)

‡ On this range rises a peak sufficiently high to be *always* enlightened by the Sun and so to enjoy perpetual day, while the plains on the north base experience as constantly the ever-recurring vicissitudes of day and twilight (B. & M.)

§ Positive and negative photographs were taken of the Moon during last

On the north-west of the valley the elevated patch appears dome-shaped, and as *Autolycus* occupies the central position, the idea is suggested that the Ringgebirg was the ultimate result of the upheaving forces. At the north-west point above *Hadley* there is (as in the lunar map) a slight depression.

The entire Apennine region (Das Apenninen Gebirg und Hochland of B. & M.) is a large triangular district of which the promontory north-west of *Hadley* is the apex and the north-east converging side the one arresting our attention. In this side may be noticed about seventeen marked breaks, with the principal elevations standing out in high relief. Gigantic spurs (Vorberge, B. & M.), and rather high ranges are also to be seen running south-west, leaving profound intervening valleys. Beginning north, *Hadley*, *Aratus*, *Bradley A*, *Bradley*, *Huygens*, *Huygens A*, *Wolf A*, can be traced towering up more or less strikingly,—*Huygens A*, the highest (about 21,000 feet, B. & M.)

The physical characters of this part of *Apenninus* are those of a mountainous region, abruptly terminating on one side, running for a great distance in a course very direct, with subordinate ranges branching off at right-angles to the coast-line. These features present a structure very similar to that displayed on good maps of the Himalayan chain, where the great spurs and the lesser elevated lines of hills, are at right-angles to its main axis, while the sudden fall of *Apenninus*' coast-line to the level of *Mare Imbrium*, is also analogous to the Asiatic breakdown of the high land of Thibet to the low level of Hindostan.

It is as important to the selenologist as it is to the geologist to obtain some approximate knowledge of the chronological sequence of the different formations (or modifications) of the lunar crust. The general appearance of *Mare Imbrium*, running up direct (and to so great an extent unchanged in area level) to the very foot of *Apenninus*, would lead to the inference that their physical condition existed anterior to the more local and limited upheaval of the small *patches north and south* of the valley of *Archimedes*. The *north patch* indicates that expansive forces had first produced the domical form, and then ultimately found vent in the ejection and building up the crater of *Autolycus*. These are symptoms of continuous, or at least, of protracted igneous action. *Archimedes* is probably posterior in age to the *south patch*, because the great ridging and elevation of high hill ranges seem to be rather connected with his protrusion. Again while it is clear that the *Apennine* range ran up to and beyond *Eratosthenes*, it is also probable that it once extended to *Copernicus*. If so, it would then follow that the elevation of this most magnificent Ringgebirg with its radial system, would, *pro tanto*,

month, and at the full Moon of this. On cursory views both showed decided triangular shading, but the translucent glass positives (much magnified) viewed both on collodionized and non-collodionized surfaces, and under different angles of light (*over black velvet*) distinctly showed the details of the *Archimedean* valley and the *south-west patch* ridged with hill ranges.

have obliterated the continuity of this range (as appears to be the case), and consequently be newer in age. On tracing carefully the direction of one of the north rays of *Copernicus* we find it to bifurcate near the *south patch*, and one of the branches to touch and become lost in or under it. There are often great niceties in judging of the effects of these rays, and inferences must be drawn cautiously, but, on the whole, I think that this branch underlies without ridging the patch. The connection of the raised parts with the branching and curvilinear elevations being already hypothetically accounted for, it would then follow that the ray is older than the south patch. Lastly, as to the remaining question of the relative ages of the *Apennine* range and *Eratosthenes*, I should consider this Ringgebirg as last formed, because its circular shape gives evident proof that nothing has interfered with its form since its upheaval, while the *Apennine* range seems to have been clearly interrupted by *Eratosthenes*. Hence, I infer the relative ages of these localities and districts to stand thus in a descending series (beginning with the newest), *Autolycus*, *Archimedes* (and perhaps the hill ranges on the south patch), *north and south* patches, *Copernicus*, *Eratosthenes*, *Apennine* range, *Mare Imbrium*.

Ensleigh Observatory, Lansdowne, Bath,
27 *Sept.* 1869.

On his new Observatory at Churt, Surrey.
By R. C. Carrington, F.R.S.

I have bought the freehold of almost nineteen acres of land situated six miles to the south of Fareham, on a part of Frensham Common, in the village of Churt. It contains a conical hill, sixty feet high, which is entirely detached, and it was this that induced me to purchase it. It goes by the name of The Middle Devil's Jump, as in another spot near by there is the Devil's Punch-Bowl. Its situation by the Ordnance Survey is

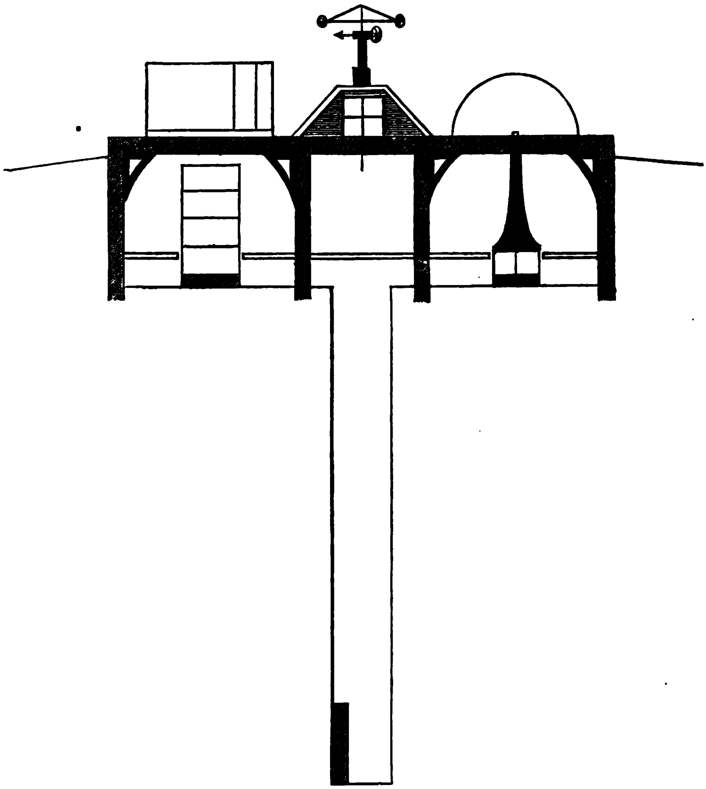
Lat. 51° 8′ 49″ N.
Long. $0^h\ 3^m\ 1^s{\cdot}7$ West of Greenwich.
Alt. about 340 feet above Liverpool.

Farnham is the nearest post-town and railway-station. It is necessary to state this, as I find letters and parcels continue to be addressed to me at Redhill, though I have left it seven years ago.

There are three things I wish to speak of, the Observatory in itself, the clock, and the principal instrument.

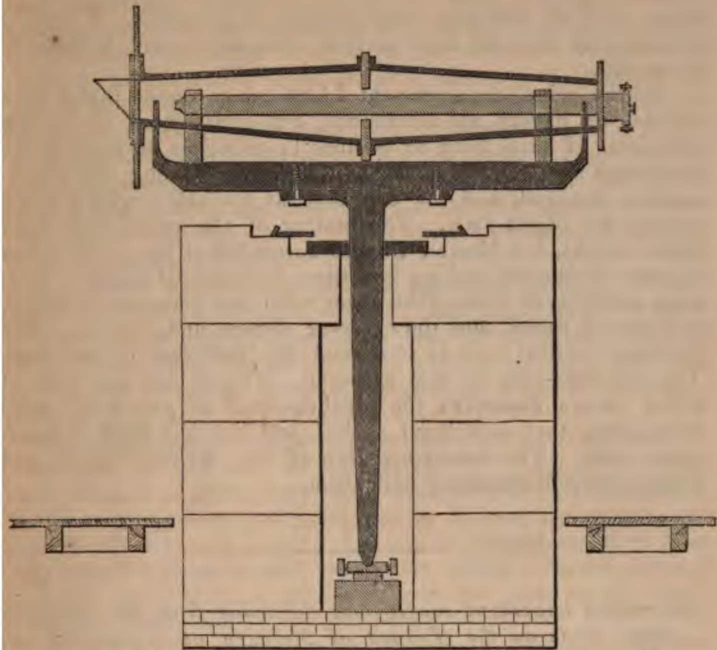
Being on a hill I did not want elevation, so I have sunk the

Observatory below ground, just peeping out over the soil. But I have further sunk a dry well, six feet in diameter, to the depth of forty feet from the centre of the Observatory, and with a horizontal shaft communicating with the south side of the hill, 166 feet in length, closed with three doorways. This is principally intended for the clock, for I am determined that one clock at least shall be properly mounted, at a position of invariable temperature and in an air-tight case. I propose to reduce the pressure to twenty-seven inches of mercury. My model clock is at present the one at Bidstone Observatory, made by the late



Observatory.

Mr. Richard Bond, with a gravity escapement, and its rates vary not more than $0^{s}.04$ per diem in November and December. Still Mr. Frodsham, to whom I applied, assured me that, provided I had a good pendulum, he did not care what the escapement was, and I have reluctantly gone back to the old dead beat. The communication with the moveable dial above is by galvanic



Altazimuth. Dark shade steel or cast-iron, light brass or gun-metal.

Altazimuth viewed from above. Scale $\frac{1}{10}$.

B

wires, and all difficulty about hearing is overcome. I hope I shall shortly have the most perfect clock in England, perhaps in the world.

Of the principal instrument I have to announce that I have decided on having an altazimuth, but constructed on a new principle. I have fixed on Steinheil's principle of making the horizontal axis the effective optical axis, by placing the object-glass at one end, and the eye-piece at the other, with a prism outside the object-glass. The casting of the prism took over three months, but Messrs. Chance succeeded at last; six inches aperture is adopted, and six feet three inches focal length. It is some comfort to think that never need the telescope be raised, only turned round, and the observer always under cover. The principal vertical axis is steel, and the bed-plate of cast-iron. The telescope-tube is also cast-iron. The circles are both of thirty inches diameter; the altitude-circle of gun-metal solid throughout, that is without spokes, and cut out from a much larger piece. The microscopes are of five feet focal length, and I hope to try photography with them.

On certain important conclusions deducible from the Observations made on the Transit of Mercury at Greenwich, on November 5th, 1868. By Richard A. Proctor, B.A.

From a careful study of the investigations to which the observations made on the transit of *Venus* in 1769 have been subjected, and more especially of the masterly researches of Mr. Stone in the *Notices* for October 1868, I came long since to the conclusion that one of the chief points to be considered by astronomers in preparing for the coming transits is the effect which has been termed the "clinging of the limbs of the Sun and planet" near the time of true internal contact. In an article which appeared in the *Daily News* of November 4, 1868, I called special attention to the advantages which might result to science if observations were made on the internal contact of *Mercury* with reference to this effect. I wrote as follows: "Though transits of *Mercury* are not in themselves very important phenomena, it cannot be doubted that astronomers will avail themselves of the opportunity to practise, so to speak, for the approaching and far more important transits of *Venus* in 1874 and 1882. They will inquire whether the magnifying power of the telescope made use of has any bearing upon the duration of the deceptive appearance, or whether darkening glasses somewhat more powerful than those usually employed may not diminish the irradiation to which the phenomenon is due." A week later I wrote a letter to the same effect to the editor of the *Scientific Opinion*, and in a paper which appeared in that journal yet a week later (but was

in fact written at the same time as the letter), I called particular attention—first to the bearing of Mr. Stone's re-investigation of the transit-observations of 1769 on the subject of the coming transits ; and, secondly, to the probable value of a "well-concerted plan of observations or even of a comparison, *inter se*, of observations already made on the peculiarity in question."

I mention these facts to explain the interest I take in the valuable series of observations made at Greenwich on the transit of *Mercury* in 1868, and particularly in the examination to which Mr. Stone subjected those observations. Certain highly important consequences seemed to me so obviously to flow from Mr. Stone's paper on the subject in the *Monthly Notices* for November, 1868, that in all cases where in treating the coming transits of *Venus* these consequences have been in question, I have in effect taken them for granted, not wishing to waste space in pointing out what Mr. Stone had already, as I conceived, demonstrated.

But as I judge from a passage in Mr. Stone's last communication to the Society that he himself reads his observations differently than I had been disposed to do, I feel that the importance of the whole subject will justify me in inviting attention to the conclusions which may, as I conceive, be directly deduced from Mr. Stone's "Remarks and Suggestions arising from the observations of the Transit of *Mercury* across the Disc of the Sun." (*Monthly Notices* for November, 1868.)

I refer now principally to the remark in his last paper, that the errors of contact-observations, "arise when we assume that the phase of the phenomenon observed by one observer corresponds to the phase observed by another observer in such a way that each takes place at the same distance between the centres." I had so read Mr. Stone's earlier paper as to believe its main object was to *prevent* observers from falling into this sort of error; and I still, after renewed and careful study, not only of that paper, but of the whole subject, can form no other opinion than that the practical value of Mr. Stone's researches is altogether greater than he seems to believe. Instead of looking upon his remarks as merely suggesting a difficulty, I find in them the clear indication of a method of escape from that difficulty.

To save space, I will take only the extreme cases of difference referred to in Mr. Stone's last paper.

Mr. Lynn, observing with the north equatoreal and power about 170, saw a delicate filament form itself between *Mercury* and the Sun's limb at $21^h\ 0^m\ 0^s{\cdot}7$ G.M.T. The filament was so fine that we may believe that Mr. Lynn really "caught a phase very near that of real internal contact."

Mr. H. Carpenter, using the east equatoreal and a power of 70, observed the sudden formation of a broad ligament between *Mercury* and the Sun at $21^h\ 0^m\ 11^s{\cdot}1$ G.M.T.; and on Mr. Stone's requesting him to point out which of certain figures (see *Monthly Notices* for November 1868) "appeared to him to represent most

nearly the appearance of the planet at the time of his observation, he without hesitation pointed out fig. (3)," but the phase, Mr. Stone says, was "somewhere between (2) and (3)."

Now in fig. 2 of the above-named paper, the ligament is rather less than one-half the diameter of *Mercury* in width, in fig. 3 rather more than one-half. We may suppose that in reality it was as nearly as possible one-half.

Therefore, supposing we wished to determine the time of real internal contact from Mr. Carpenter's observation, we ought, I apprehend, to proceed as follows.

We may assume, first of all, that irradiation does not tend to make the cusps seem appreciably nearer together than they actually are, because on no other hypothesis can we account for their squareness of outline. It follows, then, that the chord joining the cusps at the time of Mr. Carpenter's observation was equal in length to about one-half the diameter of *Mercury*. Hence it subtended at *Mercury's* centre an angle of about 60°, and therefore *Mercury* had really crossed the Sun's limb (normally) by a distance equal to *Venus*' radius × vers. 30°, approximately.

Now in the transit of Nov. 1868, *Mercury* traversed an arc of 41°, from external contact to external contact (Sun's semidiameter $16' \; 10''\cdot 7$, *Mercury's* $4''\cdot 9$) in $3^h \; 38^m \; 18^s$. And it is easy to calculate from this *Mercury's* normal velocity, which I find was $0''\cdot 06412$ per second. Hence the interval which had elapsed between real contact and the time when Mr. H. Carpenter observed the formation of the broad ligament, was

$$= \frac{4\cdot 9 \,.\, \text{vers}\, 30^\circ}{0\cdot 06412} \text{ seconds of time,}$$
$$= 10\cdot 3 \text{ seconds.}$$

Hence the calculated epoch of real internal contact is—

$$21^h \; 0^m \; 11\cdot 1^s \text{ G.M.T.} - 10^s\cdot 3$$
$$= 21^h \; 0^m \; 0^s\cdot 8 \text{ G.M.T.}$$

Mr. Lynn's observation gives, for the same phase—

$$21^h \; 0^m \; 0^s\cdot 7$$

Hence, when thus treated (and there is nothing forced in the above method, nothing, in fact, which is not strictly accordant with Mr. Stone's interpretation of the nature of the clinging), Mr. Carpenter's observation gives a result *differing by only one-tenth of a second from Mr. Lynn's.*

I do not suppose for a moment that the closeness of agreement in this particular case is not in part accidental. But it appears to show very clearly that we can, by treating in the same way observations made on *Venus* in 1874 and 1882, obtain results *much* more accurate than by neglecting the considerations flowing from Mr. Stone's remarks on the transit of *Mercury*.

Of course, if those who come to manipulate the observations of

1874 and 1882 should insist on assuming that a phase caught by one observer is identical with a different phase caught by another, the old cause of errors will creep in. But in doing so they would be blinding themselves to the real value and significance of the observations made at Greenwich on the morning of November 5, 1868. This will not happen I should imagine. If, however, an Encke of the future should make such a mistake, doubtless there will not be wanting a Stone of the future to correct the results so obtained.

So long as the observers in 1874 and 1882 indicate as closely as they can the apparent breadth of the dark ligament, it must always be possible to determine the moment of real contact much more exactly in the manner above indicated than by assuming the phase observed to be actually a contact. It is for this reason that I have been always unwilling to accept the view that the probable error will be proportional to the slowness of the planet's normal velocity. I believe, on the contrary, that, during a transit of slow normal velocity, the time given to the observer to form several estimates of the breadth of the ligament, will counterbalance (not wholly, but to an important extent) the absolutely greater time-intervals which separate different phases of the internal contact. I have not before entered at length into the reasons which led me to this view, because they seemed so clearly deducible from Mr. Stone's valuable remarks on the transit of *Mercury*.

In the preceding paragraphs I have referred only to ordinary eye-estimates of the breadth of the dark ligament. It seems so obviously suggested that the eye-piece of each telescope made use of should be provided with an arrangement for estimating the breadth of the ligament — as compared with the diameter of *Venus*, that perhaps it may seem unnecessary that I should mention the point. Still, as I have found hitherto no reference to the necessity of this being attended to, I may be excused for remarking on it.

Micrometrical measurement is, perhaps, not desirable, though it would clearly be possible to apply it in an effective manner. It is not necessary, however, because absolute dimensions are not required. All we want is to know the ratio which the breadth of the ligament bears to the diameter of *Venus*. Hence if we adopt any of the numerous contrivances by which a series of cross-lines (or two series at right angles to each other would be even better) can be made to appear in the field of view with *Venus*, it will be the simplest possible matter for the observer to determine the relative breadth of the ligament — and this, even though at the moment of contact neither set of cross-lines should be absolutely normal to the Sun's limb at the place of contact.

If it should be thought advisable (as was suggested in November 1868 by the Astronomer Royal) that places of observation should be so selected that *Venus* will cross the Sun's limb either near its highest or near its lowest point, all that is requisite is,

that the place of observation should be taken as near as possible to the curved diametral arrow which lies across my two charts of the Earth in the *Monthly Notices* for June 1869 (plates 5 and 6). Most of the stations already dealt with are little affected by this condition; but, for observing retarded egress, all the Indian stations will be found (in this respect) far better than Alexandria. At Peshawur, for instance (a place already superior as respects coefficient of parallax and solar elevation), *Venus* will leave the Sun almost exactly at his uppermost point, whereas at Alexandria the point of last contact will be about 34° from the uppermost point of the Sun's limb.

A New Theory of the Milky Way. By R. A. Proctor, B.A.

Sir W. Herschel's respect for existing analogies—a quality which is perhaps of all others the safest guide for the scientific explorer,—led him to adopt as the means of interpreting his noble series of star-gaugings the hypothesis that there is a general uniformity in the distribution of the stars through space. He adopted this hypothesis not from a conviction of its being actually true, nor even from the belief that it is approximately so, but simply because existing analogies seemed to render it probable, and because it formed a convenient basis for calculation. The existing analogies were those presented by the solar system. In this system, Sir W. Herschel recognised a number of discrete bodies, not equal indeed, but comparable *inter se* in magnitude; not uniformly distributed, but still not aggregated towards one or another part of the solar scheme. And making such modifications as seemed requisite in comparing a system not regulated by a vast central orb with a scheme like our solar system, it seemed likely to him that a general equality of magnitude, and a general uniformity of distribution, might be found to prevail among the members of the sidereal system.

We now know that the ideas which astronomers had formed of the solar system in Sir W. Herschel's day were very far indeed from being correct. We see in the solar system a complexity of detail, and a variety of form, structure, aggregation, and motion, which were altogether unknown a century ago. And I cannot doubt that if the view we have of the solar system had been presented to Sir W. Herschel, he would have adopted as the basis of his star-gaugings an hypothesis differing altogether from that of which he actually availed himself. He would have argued, that as, in the solar system, there are bodies like the planets, far surpassing the other members of the scheme in magnitude and in importance; as it contains zones of minute bodies, such as the asteroids and the satellites composing the rings of *Saturn;* myriads of meteoric systems, and countless thousands of cometic systems, so doubtless in the sidereal system there are many forms of matter. If the analogy of the solar system is to be our guide, we

must look for suns equalling or surpassing our own in magnitude and splendour; for clusters and systems of minor suns, whose united mass may fall short of the mass of one of the primary stars; for aggregations of matter in portions relatively so minute as not even to be comparable with the small stars found in true star-clusters; and finally, for systems composed of materials, or at least of forms of matter, differing as widely from the substance of the suns, as the matter composing a comet does from the substance of the Earth or of *Jupiter*.

But, even independently of analogies such as these, his own series of observations led Sir W. Herschel to feel more and more doubt, as he proceeded, respecting the hypothesis which he had made the basis of his calculations. It is only necessary to compare the later papers he sent in to the Royal Society with the earlier ones, to find that views altogether inconsistent with his initial hypothesis were opening out before him. It was in those later papers that he spoke of star-groups in the Milky Way clustering towards opposite regions of the heavens; of stars arranging themselves into separate systems; and of the signs which the heavens present of the action of processes of aggregation, causing "the gradual dissolution of the Milky Way."

Sir John Herschel, also, in carrying out the system of star-gauging among the Southern stars, was led to notice many features which the hypothesis of a tolerably uniform distribution of stars could not satisfactorily explain.

Judged according to Sir W. Herschel's fundamental hypothesis, the sidereal system came to be regarded as forming a figure resembling that of a *cloven disc*, and the Milky Way was explained as being due simply to the greater extension of the system in the direction of the medial plane of this disc. Sir John Herschel, however, from his observations of the Southern heavens, was led to suspect that this theory was not strictly correct. He speaks in one place of certain evidence, according to which the Milky Way would come to be regarded as a flat ring seen edge-wise. And in many places he speaks of the difficulty of understanding certain features according to the views usually accepted.

It seems to me that the evidence collected respecting the Milky Way is sufficient to lead us to quite another view of its structure than that to which Sir W. Herschel was led by an hypothesis founded on the incomplete theories which astronomers in his day had formed respecting the solar system.

Let us regard the matter altogether independently of preconceived opinions, and judge simply as the evidence may seem to teach us.

In the woodcut, the outer figure represents the Milky Way according to the drawings and description of Sir John Herschel. The mode in which it is delineated needs no explanation.

Now in regarding this picture of the Milky Way, we are forced, I think, to the conclusion that neither the cloven-disc theory, nor the flat-ring theory, accounts satisfactorily even

for the principal features of the Milky Way. For example, the great gap which crosses it in *Argo*, nearly in the widest part of the single branch, seems utterly inexplicable on either theory. There is no way of accounting for that gap if we are really supposed to view the Milky Way from a point within its figure, and that figure resembles—however roughly—either a cloven disc or a flat ring.

But let us pass to other features. Travelling towards the right from the gap, we come to the strange semicircular cavity

with a well-defined outline, which Sir John Herschel has described in such striking terms. A cavity of that figure is a remarkable phenomenon, and is surely inexplicable either on the flat-ring or cloven-disc theory. But the mere distinctness of the outline is one of the strongest possible proofs that the stars which form that portion of the Milky Way constitute a distinct clustering aggregation from which we are separated by an enormous and comparatively star-less interval.

We come next to the Great Coal-sack near *Crux*, almost opposite to which is a well-marked opening in *Cygnus*. There are

also other strange openings through different parts of the Milky Way.

Now I cannot but think that an argument similar to that which Sir John Herschel has applied with so much force to the Magellanic clouds applies to the openings in *Crux* and *Cygnus.* He argues that because the Magellanic clouds approach roughly to the circular figure, therefore, in all probability, their real figure is that of a sphere: the chance is small that one of them is a cylindrical system seen endwise, but the chance that both are is altogether evanescent. Now applying the same principle to the Coal-sacks, we are led with equal certainty to the conclusion that these apertures are not cylindrical or tunnel-shaped openings seen endwise, but *if they are really openings at all* they are openings through a system which is not very much deeper—measured in the direction of the line of sight—than the greatest width of the aperture itself.

Judged in this way, the parts of the Milky Way which lie round a "Coal-sack" would have a roughly circular section, and not that enormous extension in the direction of the line of light, which has been assigned to them.

I cannot see that this argument is at all less sound or less effective than that which has been applied by Sir John Herschel to the Magellanic clouds.

There is another feature referred to, and I believe discovered, by Sir John Herschel, which is also full of meaning. I refer to the existence of narrow and sometimes convoluted streams of stars, branching out from the Milky Way itself. Sir John Herschel says of these that we ought to look on them as in all probability planes or scrolls of stars seen tangentially, and not as branch-shaped extensions bristling up from the general level of the Milky Way. And undoubtedly if the Milky Way really have a great extension in the direction of the line of sight, it is just that we should so regard these outlying streams. But if we judge of them without any reference at all to pre-existing theories, we are guided by strong arguments from probability to form a very different view. The chance that a plane system of stars, and still more a scroll of stars, should be turned so directly towards the Sun as to present to us the appearance of a straight or convoluted line or narrow stream of stars, is small indeed. The probability that several should be so situated may be regarded as evanescent.

Accepting these streams as having a roughly circular section, we are led to the conclusion that the Milky Way from which they extend has a similar section. In fact, as Sir John Herschel held these streams to be really planes or scrolls because (I assume) he assigned to the Milky Way a great lateral extension, so by inverting this argument I am led to believe that the Milky Way has not a great lateral extension (compared I mean with its thickness), because the streams extending from it have in all probability a section of roughly circular figure.

Other arguments there are that space will not permit me to dwell upon here, which point in the same direction.

Now, of course, if the Milky Way forms in reality a stream of stars *amidst the sidereal system*, the appearance which it presents upon the heavens might be expected to afford some information as to the shape of that stream, or at least of that portion which is cognisable by us. It must be admitted, however, that the problem of interpreting this wonderful stream is one of enormous difficulty. Perhaps it is one which men will never be able to accomplish in a perfectly satisfactory manner. It is only necessary to contemplate that marvellous maze of star-streams around *Scorpio* and its neighbourhood, and to read the account which Sir John Herschel gives (in his *Cape Observations*) of the telescopic aspect of this region, to feel how far we are at present from being able rightly to interpret the mysteries of the Galactic circle.

The bolder and more striking features of that circle may, however, be studied with a better hope of their being successfully interpreted. A theory which will explain the gap in *Argo*, the wide break of one stream in *Ophiuchus*, the varying brightness of the principal stream in different parts of its length, and other features of this kind, may reasonably be sought for.

I have endeavoured in the inner circle of the figure to indicate a spiral which seems to me to account for the most striking features of the Milky Way.

Following that spiral round from the part where the two loops approach each other, we have the following relations :

First of all, the gap is explained by the fact that the two loops do not meet. Then, remembering that the spiral is supposed not to lie in one plane, but (as the contorted figure of the Milky Way obviously suggests) to have been swayed out of that plane by varying attractive influences, we see that where the line of sight is directed tangentially to either loop, the Milky Way might be expected to have greater width than elsewhere. This explains the fan-shaped expansions on each side of the gap. Then on each side of these expansions we see the Milky Way double, which obviously corresponds to the relations exhibited by the two loops. Following the wider loop, we see that the double part of the Milky Way on this side extends nearly through a complete semicircular arc. The Coal-sack is explicable as due to the apparent intercrossing of the two contorted streams which really are at different distances from the eye.* The break in the further branch seems readily explicable as due to the great distance of a portion of this branch. But here the theory derives a singular support from the actual relative brilliancy of different parts of the Milky Way in this neighbourhood. Every astronomer knows how strangely the light of the Milky Way varies in and near

* In the large maps of the S. D. U. K. the Milky Way is depicted near *Crux* and *Argo*, as if the object of the draughtsman had been to support my theory. In Sir John Herschel's drawing, however, there are no such varieties of brilliancy.

Cygnus. The branch which extends from the Northern Coal-sack towards *Albireo* is at first far the brightest, and then fades off so much that in *Ophiuchus* it is wholly lost. The other branch, on the contrary, gradually increases in brightness, until in *Aquila*, and, further on, in *Sagittarius*, it forms the brightest part of the whole Milky Way. Now this part which is so very bright, corresponds to the part which my spiral brings so very near to the Sun.

Passing on to the termination of the second branch near *Cygnus*, it will be noticed how the spiral explains the strange extension of milky light from *Cepheus* towards the north pole.

Thence the stream is single, growing gradually fainter with increase of distance towards *Canis Minor* and *Monoceros*.

The spiral I have depicted seems so satisfactorily to account for several of the more striking features of the Milky Way, as to suggest the idea that it probably corresponds somewhat closely to the real figure of that star-stream. I am sensible, however, that many peculiarities remain unexplained by, though they are by no means opposed to, my theory. It must be remembered that any objections founded on a presumed equality of stars throughout the Milky Way, or of a general uniformity of distribution throughout the spiral stream, do not require to be met; because at the very beginning of this inquiry I have abandoned such hypotheses as inconsistent with existing analogies.

For example, there may be parts of this Milky Way so constituted, that if we were to remove further and further from them, we should see them gradually assuming the form of irresolvable nebulosity. But there may be other parts which would never assume that appearance, let their distance be what it might — the distribution and magnitude of the component stars being such that the stars would vanish through effect of distance, before the distances apparently separating them became evanescent.

I may add as a striking confirmation of a portion of these views, that among the lucid stars along the part of the Milky Way which lies nearest to the Sun, according to my view, are those which have been actually found to be nearest to us.

It must be understood that I regard the Milky Way as simply the condensed part of a spiral of small stars, which has been swayed into its present figure by the influence of large stars — the lucid stars seen in the Milky Way. The myriads of small stars not lying in or near the Milky Way, must yet belong to the same system, and in some instances seem to obey somewhat similar laws of aggregation. The nebulæ, so far as the evidence from probability extends, would appear to be groups formed from among those stars that have not fallen under the influence of the large stars which have brought the Milky Way spiral to its present figure. In the Magellanic clouds, we see the action of processes which have tended to form spherical clusters of enormous dimensions, in which both forms of aggregation are met with.

Why, in different parts of the sidereal system different pro-

cesses of aggregation should have taken place, we cannot yet distinctly see. But some of the striking discoveries which have recently been made by astronomers afford promise that light will soon be thrown on these perplexing questions.

P.S.—If my views respecting the Milky Way are correct, it obviously follows that there are parts of the Milky Way where traces of annual parallactic displacement might be looked for amongst *telescopic stars.* One instance of such motion would force us to modify all the views at present accepted respecting the sidereal system.

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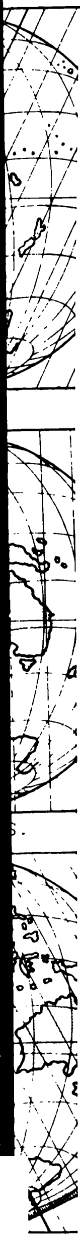
ENUS'S



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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *January* 14, 1870. No. 3.

WARREN DE LA RUE, Vice-President, in the Chair.

J. M. Eustace, Esq., Wimbledon;
J. E. H. Peyton, Esq., 63 Chester Square;
W. Garnett, Esq., Bashall Lodge, Clitheroe;
Capt. J. Williams, Aberdeen; and
Lieut. H. F. Yeatman, R.N., Sherborne, Dorset,

were balloted for and duly elected Fellows of the Society.

On the Eye-piece for Correction of Atmospheric Dispersion. By G. B. Airy, Astronomer Royal.

In a late Number of the *Monthly Notices*, I showed that the effect of atmospheric dispersion on the images of celestial objects viewed at small elevations (to which I may add, the chromatic separation produced by slight error in the centering of the lenses of an object-glass), might be corrected by the insertion of a prism of proper angle in the eye-piece. It was obviously an inconvenience, though a very trifling one, that a battery of different prisms must be prepared for different elevations of the object viewed; and it would be better, if possible, to avoid the insertion of an additional piece of glass.

Mr. William Simms pointed out to me that a prism of adjustible angle would be produced by causing the convexity of a plano-convex lens to roll in the concavity of a plano-concave lens,

the radii of curvature of the convex and concave surfaces being equal. This construction would relieve the first of the inconveniences which I have mentioned, but would aggravate the second.

After various suggestions, Mr. W. Simms and myself arrived, independently and simultaneously, at a construction which, for simplicity and optical perfection, is not likely to be improved. It consists merely in making the eye-glass (supposed to be plano-convex) broader than is strictly necessary for the telescopic vision, causing it to press by its convex surface into a concave cup at the eye-end of the eye-piece, and allowing it to roll in that concavity; thus presenting different parts of its convex surface, though always in the same form and position, to the rays of light which come from the field-glass, but presenting to the eye a plane

Fig 1.

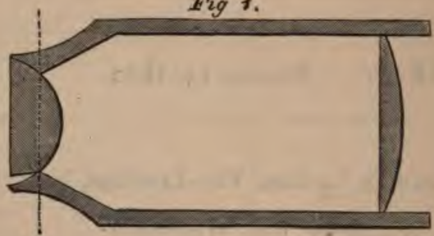


Fig 2.

surface, which, in one position of the lens, is normal to the telescope-axis, and, in another position of the lens, is inclined to it.

The above diagrams will explain the optical action of such an eye-piece.

Fig. 1 shows the position of the lens in the state for ordinary use.

Fig. 2 shows its position when atmospheric dispersion, &c., are to be corrected.

It will be seen that, in each case, the dotted line separates, on the telescope-side, a plano-convex lens of definite form; but that, in the first case, there is applied to it on the eye-side a *piece of glass* bounded by two parallel surfaces; and in the second

case, there is applied to it a prism, whose angle is gradually varied by gradually varying the position of the lens in its cup.

I have represented the large eye-lens as incomplete towards the lower edge. Optically, no injury is produced by making it complete; but I thought it possible that the projection of the lens edge might be inconvenient to the observer's eye; and also that the bluff termination of the lens might be convenient for the application of screw-motion.

The eye-piece in this state presents these advantages: it introduces no additional glass; it allows the use of a prism with angle gradually changing; and it does not disturb the corrections for spherical aberration.

It seems not too much to say that every telescope intended for delicate purposes might advantageously be furnished with such an eye-piece.

The eye-piece must have a swivel or rotatory motion round the axis of the telescope. A very simple screw arrangement, in a specimen eye-piece furnished to me by Mr. Simms, appears to place the eye-glass entirely under command.

1869, *Dec.* 20.

Seventh Catalogue of Double Stars, observed at Slough, in the years 1823–1828 *inclusive, with the* 20-*feet reflector;* 84 *of which have not been previously described.* By Sir J. F. W. Herschel, Bt. F.R.A.S. (Abstract.)

The observations of double stars herewith submitted to the Royal Astronomical Society were made in the course of my earlier sweeps at Slough, with the 20-feet reflector, and would have been included in the third of those Catalogues of double and triple stars observed with that instrument which the Society has already honoured with a place in its Transactions, but for a twofold reason, viz., that that Catalogue was limited, 1st, to stars not before (to my knowledge) observed as double; and 2ndly, to the completion of a first exact thousand of such objects; thus causing the exclusion of 84, which are here entered as Nos. 5450–5533, and which in the subsequent redaction of Catalogues 4, 5, and 6, would appear to have escaped entry, owing to non-advertence to this circumstance. The remainder of the present list is made up, for the most part, of stars included in Struve's Dorpat Catalogue, and recognised as such. Although the angles of position of these latter stars are given only from careful estimation, they are not without some considerable historical value, inasmuch as in a great many instances they are antecedent in point of date to any recorded measurements, and in several are the only existing records of position; and though in some particular cases widely erroneous, they yet present for the most part such an agreement with subsequent micrometrical measures as in the earlier stages of this branch of astronomy would have ren-

dered their evidence available in deciding on the probability or improbability of a binary connexion between the individuals. Having included them, moreover, among the data which I have been for a long time occupied in collecting and arranging in a general Synoptic History of these objects; a reference to some recorded collective statement of them accessible without recourse to the original sweeps became unavoidable.

Summary of Sun-spot Observations made by the Kew Photo-Heliograph during the year 1869.

(*Communicated by Messrs. De La Rue, Stewart, and Loewy.*)

The following table exhibits our annual *resumé* of Sun-observations made at the Kew Observatory, drawn up according to the plan of Hofrath Schwabe, in Dessau:—

Months.	Days of Observation.	Days without Sun-Spots.	Number of New Groups.	Nos. given to the New Groups in the Kew Catalogue of Sun-Spots.	
January	14	0	15	No. 902	to 916
February	15	0	17	917	933
March	11	0	14	934	947
April	20	0	15	948	962
May	16	0	18	963	980
June	18	0	27	981	1007
July	22	0	18	1008	1025
August	19	0	25	1026	1050
September	21	0	21	1051	1071
October	18	0	17	1072	1088
November	11	0	15	1089	1103
December	11	0	22	1104	1125
Total	196	0	224	No. 902	1125

Remarks.—The steady increase in the number of groups and the immense areas which many of them covered, points to the approaching maximum of the sun-spot period.

The year was also characterised by a remarkable tendency of the groups to appear in successive trains, within narrow and well-defined zones on both sides of the solar equator. Such a regular successive appearance of spots along parallels of latitude had previously been observed, but it usually only lasted during a short period, after the lapse of which the distribution in latitude became again irregular. Last year the irregularity of distribution was rather the exception. It is not improbable that a distinct law may be traced at some future time in this singular behaviour, and we recommend the subject to the attention of observers.

Kew Observatory, January 1, 1870.

On the Determination of the Orbit of a Planet from Three Observations. By Prof. Cayley. (Abstract.)

The author has proposed to himself to consider from a geometrical point of view the problem of the determination of the Orbit of a Planet from Three Observations. The Orbit is a conic, having the Sun for a focus, and each observation shows that the Planet is at the date thereof in a given line; we have thus a given point or focus, S, and three given lines, say the "rays." The orbit-plane, if known, would, by its intersections with the three rays, determine the three positions of the planet: that is, we should have the focus and three points on the orbit, or, what is the same thing, three radius-vectors from the focus, say a "trivector": geometrically, through three given points and with a given focus, there may be described four conics, but (as explained in the Memoir) only one of these can be the orbit; the orbit is thus determined, and that uniquely, by means of a given trivector. The problem therefore is to find the orbit-plane such that in the orbit determined by means of the trivector the times of passage between the three positions on the orbit may have the observed values, or (what is the same thing) that the orbital areas, each divided by the square root of the *latus rectum*, may have given values. Instead of the orbit-plane, the author considers the orbit-axis (that is, the line normal to the orbit-plane at the point S), or rather the intersection of this line with a sphere about the centre S, say the orbit-pole. To a given position of the orbit-pole there corresponds as above a determinate orbit, and the problem is to find the position of the orbit-pole such that in the orbit belonging thereto the times of passage may have given values, as already mentioned. The required position of the orbit-pole may be obtained as the intersection of two spherical curves, one of them the locus of the orbit-pole when the time of passage between the first and second points on the orbit has its proper given value, the other the locus where the time of passage between the second and third points has its proper given value; and in connexion herewith other isoparametric loci present themselves, for instance, the iseccentric lines, or loci of the orbit-pole such that along each of them the excentricity of the orbit has a given value. The object which the author has proposed to himself in the Memoir is the discussion of the configuration &c. of these loci. He considers, in the first instance, any three given rays whatever, but in the ulterior discussion of the spherical curves, which is carried out numerically, he has confined himself to the case of a particular symmetrical position of the three rays; viz., these are taken to be lines each at an inclination of 60° to a fixed plane through S; and such that their projections on this plane form an equilateral triangle having S for its centre, and that each ray cuts the plane in the mid-point of the corresponding side of the triangle. The results are exhibited graphically by means of the figures called spherograms, each the representation (on the stereographic projection) of the

half-surface of a sphere (not a hemisphere in the ordinary sense of the word, for there is great advantage in employing a different form of boundary)—viz., there is an *e*-spherogram, showing the iseccentric lines; and a T-spherogram showing the isochronic lines.

On the application of Photography as a means of determining the Solar Parallax from the Transit of Venus in 1874. By Richard A. Proctor, B.A.

It is impossible to read Mr. De La Rue's account of the results of careful measurement applied to photographs of the solar eclipses in 1860 and 1868 without recognising that we have in photography, as applied to the approaching Transit of *Venus*, one of the most powerful available means of determining the Sun's distance. Within the last few years, solar photography has made a progress which is very promising in regard to the future achievements of the science as an aid to exact astronomy. So that doubtless, in 1874, astronomers will apply photographic methods to the transit of that year, with even greater success than we should now be prepared to anticipate. It has therefore seemed to me that the photographic observation of the coming transit merits at least as full a preliminary inquiry as either Halley's or Delisle's method of direct observation.

The result of an inquiry directed to this end has led me to the conclusion that photographers of the approaching transit should adopt for their guidance considerations somewhat different than those which have hitherto been chiefly attended to.

It is undoubtedly true, as Mr. De La Rue has pointed out, that the photographer of the transit can readily take a large number of pictures, and by combining these, can ascertain with great accuracy the path of *Venus* across the solar disc. And by comparing the paths thus deduced for different stations a satisfactory estimate can be formed of the solar parallax. I do not wish to suggest any departure from this course of procedure.

On the other hand, it is undoubtedly true, as Major Tennant has remarked, that the greatest effect of parallax will be obtained for any two stations, when both stations, the Earth's centre, and the centre of *Venus*, are in one and the same plane. So far as those two stations are concerned, his remark is just, that it is the position of *Venus* at the instant when the stations are so situated, and not the nearest approach of *Venus* to the Sun's centre, which should be compared. And further, Mr. De La Rue's comment on this, to the effect that his method in reality includes Major Tennant's, is also correct. In fact, there can be no doubt that the position of *Venus* at the particular instant referred to by Major Tennant can be far more exactly ascertained by a reference to the complete path of *Venus* for each station than from any attempt to secure nearly simultaneous photographic records at stations far removed from each other.

But it appears to me that the method I am about to suggest, according to which the whole question will be reduced to the determination of a parallactic displacement of *Venus* on a line through the centre of the Sun's disc, is the one by which the fullest assistance will be obtained from photography; while a source of error, which has not hitherto been specially considered, will be practically eliminated.

It must be remembered that in the comparison of photographic records, whether for the determination of the path of *Venus* across the Sun's disc at a particular station, or for the comparison either of *Venus's* apparent position or of her path as seen from two different stations, the accuracy of the results will depend in part on the certainty with which two or more pictures may be brought into comparison by means of a fiducial line or set of lines. It seems certain that no method can be devised by which all chance of error from this source can be eliminated. The great point would, therefore, seem to be to render its effect as small as possible.

Now let us consider for a moment Major Tennant's proposition, as giving a convenient illustration of the effects of any error either in the position of the fiducial lines, or in bringing those belonging to two pictures into exact correspondence. Let Fig. 1 represent the result of a comparison between two photographs of the Sun. A B and C D are fiducial cross-lines common to both pictures, *a* is the centre of *Venus* for one picture, *b* is her centre for the other; and on the exact measurement of *a b* depends the determination of the Sun's parallax, so far at least as these two pictures are concerned. Now it is very obvious that if the lines A B, C D for one picture, have not been brought into perfect correspondence with those belonging to the other, the distance *a b* will be correspondingly affected. In fact, it would appear that if the usual methods for making the correspondence as exact as possible are followed, almost as large an error would be introduced through this cause alone as by errors in the measurement of *a b*, since the two processes—the measurement of *a b* and the adjustment of the sets of cross-lines—depend on the very same circumstance, the nicety, namely, with which the eye and the judgment can estimate minute quantities of about the same relative dimensions.

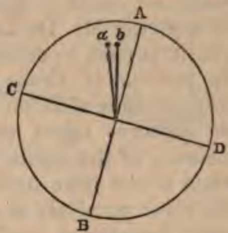


Fig. 1.

But now, if *a* and *b*, in place of having the position shown in Fig. 1, were situated as in Fig. 2, it is clear that the distance *a b* will not be appreciably affected by any small error in the adjustment of the fiducial lines.

The object, therefore, which it seems most desirable to secure is that *Venus*, as seen from two different stations at a particular instant, should have a relative parallactic displacement towards

the Sun's centre, or as nearly towards the Sun's centre as possible. This amounts to adding to Major Tennant's conditions this further one that the Sun's centre should be in the same plane with the two stations,—or rather to making this condition a substitute for that one which requires that the Earth's centre should be in the same plane with the two stations. For as a rule we must not expect to be able to secure that two convenient stations on the Earth, as well as the centres of the Earth, *Venus*, and the Sun, should be in the same plane.

Fig. 2.

Mr. De La Rue's remark that by taking a series of pictures the position of *Venus* may be ascertained at any moment is in reality quite as applicable to my suggestion as to Major Tennant's. In fact, were it not, we might despair of securing the desired object, since we have no reason for believing that astronomers are so certain as to the exact progress of the transit, that the conditions could be secured by anticipatory instructions: whereas by applying Mr. De La Rue's method it will be possible after the transit is past, to determine with any desired degree of accuracy the position of *Venus* at the proper instant. And further, it is very obvious that no error in the placing of the fiducial lines for pictures taken at the same stations can much affect the accuracy of the result, since the comparison of successive pictures, taken at the same station, does not directly involve the element of the solar parallax, as when we have to compare two pictures or paths determined at different stations.

The object, then, of the present paper and the accompanying chart is to determine what stations are most suitable for applying photography to the transit of 1874, on the principles above enumerated. I think the drawing will be found, however, to be also an instructive illustration of the whole character of the transit.

In a paper in the *Monthly Notices* for March last, I showed how all the chief elements of the transit could be deduced by con-

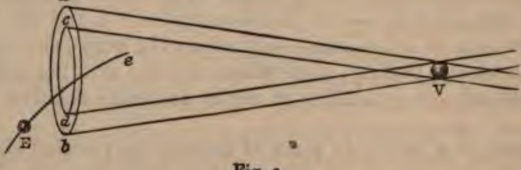


Fig. 3.

sidering the motion of *Venus* relatively to a pair of cones, each enveloping the Sun and the Earth, but one having its vertex outside the Earth, the other having its vertex between the Earth and the Sun. For my present purpose it will be convenient to con-

sider the motion of the Earth relatively to a pair of cones similarly enveloping the Sun and *Venus*.

Fig. 3 represents a portion of these cones around *Venus* at V. E *e* is the Earth's orbit relatively to these cones; *a b* is the circular section of the outer cone, *c d* that of the inner. *Venus* is supposed to be approaching the eye, and therefore the Earth also would, at the time of conjunction, be approaching; but as we are considering the Earth's motion relatively to the two cones, and as *Venus* moves more rapidly in her orbit than the Earth, we must suppose the Earth to traverse the sections *a b* and *c d* in direction E *e*. When the circumstances of the transit of 1874 are attended to, it results that the motion of the Earth relatively to the sections *a b* and *c d* is as shown in Fig. 4. The various circles represented along the parallels E *e* correspond to the various positions of the Earth represented in the illustrative plate.

Fig. 4.

Comparing Figs. 3 and 4, it will be seen at once that as soon as the Earth reaches the outer circle *a b*, external contact begins. With the peculiarities of this phase, we need not concern ourselves. When the Earth reaches the circle *c d*, as at position 1, Fig. 4, internal contact begins, and when the Earth just touches the circle *c d* on the inside, the transit has begun for all places on the Earth's illuminated hemisphere. The positions 1 and 2 correspond to the cases of internal contact most accelerated and internal contact most retarded. They have been added to the illustrative plate for the sake of completeness, but in reality they do not belong to the special subject of this paper, since, as Mr. De La Rue has remarked, the photographer need not set himself to observe special phases of this sort. The same remark applies to the positions 14 and 15.

The remaining positions of the Earth in Fig. 4, corresponding to the 11 pictures 3–13 in the illustrative plate, are those occupied by the Earth at successive intervals of 15 minutes, the picture numbered 8 corresponding to the position occupied by the Earth at $16^h\ 6^m\ 31^s$ G.M.T., on Dec. 28th 1874, when *Venus* makes her nearest approach to the centre of the Sun's disc.

Now if we look at Figs. 3 and 4, and consider what they represent, we shall see that Fig. 4 may be looked upon as exhibiting an inverted picture of the Sun's disc and the transit of *Venus's* centre across it: we see, in fact, that the apparent position occupied at any instant by any point on the Earth's surface

in Fig. 4, corresponds exactly to the position occupied by *Venus* upon the Sun's disc, as supposed to be seen from that point of the Earth's surface at the instant in question. We have only to invert Fig. 4, and look at it from behind to see what sort of path *Venus* would seem to traverse upon the Sun's disc, either with reference to the Earth's centre, or to any point of the Earth's surface supposed to be properly depicted upon the small figures 1—15.

It follows, therefore, that if we want to determine two stations at which at any instant *Venus* would appear to have a relative parallactic displacement towards the Sun's centre, all that is required is that we select two stations which are on the same radial line from the common centre of the circular sections *a b* and *c d*.

The positions of those radial lines which cross the Earth's track through the section *c d* are exhibited in the plate. It will be understood, of course, that the three rows of figures belong in reality to a single row, the numbering of the successive pictures of the Earth indicating the way in which that row would be formed by the combination of the three rows shown in the plate.

I need not explain the construction of the plate, which depends on the simplest mathematical principles. I have taken a considerable amount of care to secure accuracy, not only in the projections of the Earth, but in the position of the radial cross-lines; and, though there may be minute inexactnesses, there will be found none, I think, which affect the purpose for which the plate was constructed. What that purpose is, will be best illustrated by simply examining the indications of the successive pictures.

Passing over pictures 1 and 2, we notice in Fig. 3, that Kerguelen's Land and Crozet Island, lying nearly on a line with certain of the Aleutian Islands, suggest that pictures taken at the former stations at the beginning of the transit could be advantageously compared with pictures simultaneously, or almost simultaneously, taken at a station on one of the easternmost of the Aleutians. In like manner pictures taken near Enderby Land could be advantageously compared with pictures taken at Woahoo. Projection 4 does not differ much from the preceding, but the cross-lines have assumed a less inclined position, and Kerguelen's Land could, at the epoch belonging to this picture, be better combined with a somewhat more westerly Aleutian island.

Projection 5 exhibits the advantage of a photographic station at or near Yokohama. Probably such a station, combined with one in Crozet Island or Kerguelen's Land, would give (by pictures taken near the hour belonging to Projection 5) absolutely the best results which photography can give.

The remaining projections suggest the following combinations of photographic records:—

Projection 6. Yokohama and Enderby Land, Kerguelen's Land

and a station in Manchooria; Crozet Island and Pekin; Cape of Good Hope and Nertchinsk.

Projection 7. Kerguelen's Land and Tsitsikar; Crozet Island and Nertchinsk; Cape Town and a station west of Lake Baikal.

Projection 8. Kerguelen's Land and Nertchinsk; Cape Town and Peshawur; Repulse Bay or neighbourhood and Yokohama; Perth (Australia) and Yokohama.

Projection 9. Repulse Bay and Yokohama; Enderby Land and Nertchinsk; Crozet Island and Calcutta; Cape Town and Bombay.

Projection 10. Repulse Bay and Nertchinsk; Possession Island (near South Victoria Land) and Yokohama; Kerguelen's Land and Calcutta; Crozet Island and Peshawur; Cape Town and Teheran.

Projection 11. Possession Island and Tsitsikar; Repulse Bay and neighbourhood of Lake Baikal; Enderby Land and Calcutta; Kerguelen's Land and Madras; Crozet Island and Peshawur; Cape Town and Aden.

Projection 12. Possession Island and Nertchinsk; Enderby Land and Madras; Kerguelen's Land and Peshawur; Crozet Island and Teheran.

Projection 13. Possession Island and neighbourhood of Lake Baikal; Repulse Bay and Calcutta; a New Zealand station and Yokohama; Hobart Town and a station near the mouth of the Amoor.

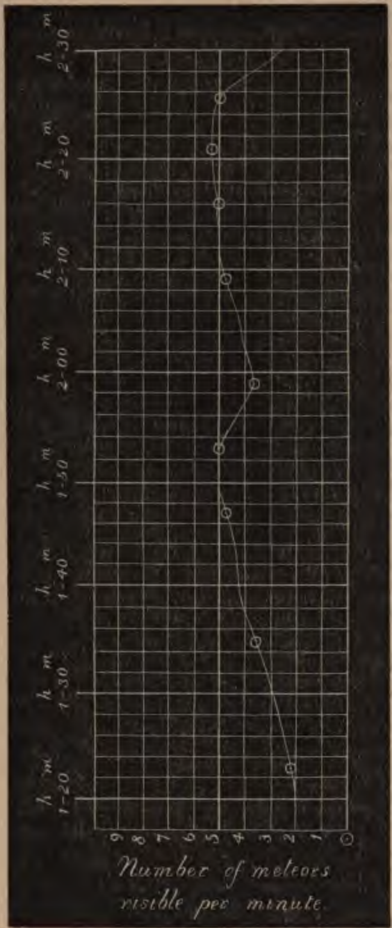
From this list we see that Kerguelen's Land and Crozet Island; Peshawur and other Indian stations; and stations in Siberia, are those which give the most favourable opportunities for the application of the photographic method.

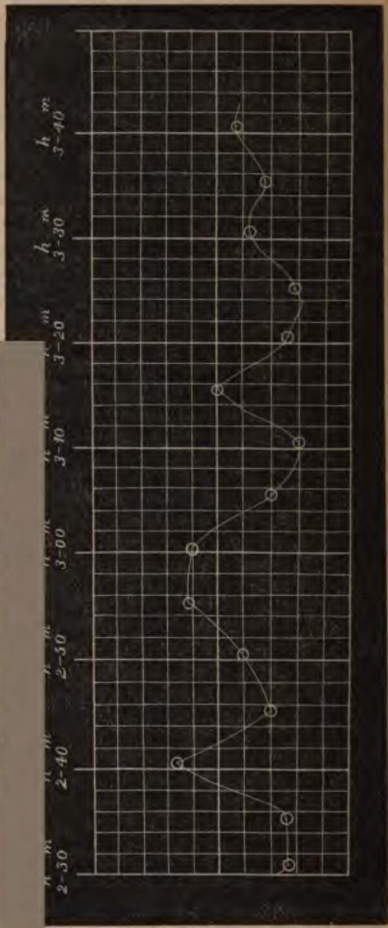
Observations of Meteors, Nov. 13–14, 1869, *at Santa Barbara, California.* Observers, G. Davidson and Mrs. E. Davidson.

Watch was kept in camp for the meteoric shower; the night beautifully clear; Moon ten days old and 4° S. Decl. I was called at $1^h\ 10^m$, up to which time 22 meteors had been seen. The diagram exhibits the numbers noted by two observers up to $3^h\ 43^m$ A.M. After that time watch was kept up for any unusual display, but the numbers gradually diminished. The total number recorded was 556 in $2^h\ 25^m$. Some of the meteors were very brilliant and left persistent trains.

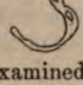
"At $1^h\ 25^m$ a meteor without apparent motion suddenly appeared, and burst in the same position. It was about 2° above and to the left of the bright star in the blade of the Sickle." (S. R. Throckmorton, Jun.)

At $2^h\ 33^m$ I observed a brilliant meteor start from a point above and a little to the left of the pointers; it left a persistent train and disappeared at a point about 9° or 10° above Polaris





From 2^h 22^m to 2^h 42^m, I was watching the persistent train of a meteor.
From 2^h 16^m to 2^h 55^m, Messrs. Throckmorton and Harford saw an average of 3 per minute.

and 6° to the right. The train was 5° in length; gradually it took a wavy form  ; then curved until it formed ⅔ of an irregular circle , and was 3° in diameter, and ½ a degree in width. I examined it with a good binocular, and found it not of uniform density, but having open spaces in it. It remained visible 8½ minutes, and in that time apparently moved in a line towards the radiant point in *Leo*, over a space of 8°.

About half-a-dozen meteors were observed moving in directions towards the radiant. G.D.

Coast Survey Station, Santa Barbara, California.
Lat. 34° 24′ N., Long. 7^h 59^m.

Occultations of Stars by the Moon.

Observed at Maresfield, in Sussex, by Capt. W. Noble.

(Previously unreported.)

Monday, September 27, 1868.

Occultation of f Tauri.

The star disappeared (though not with extreme sharpness) at the Moon's bright limb at

$21^h\ 38^m\ 40^s{\cdot}5$ L.S.T. = $10^h\ 29^m\ 35^s{\cdot}5$ L.M.T.

Reappearance not seen.

Power 154 adjusted on the star.

Friday, February 19, 1869.

Occultation of 48 Tauri.

This star disappeared instantaneously at the Moon's dark limb at

$7^h\ 17^m\ 24{\cdot}3$ L.S.T. = $9^h\ 17^m\ 59^s{\cdot}5$ L.M.T.

and reappeared at the southern part of the bright limb about

$7^h\ 47^m\ 20{\cdot}15$ L.S.T. = $9^h\ 47^m\ 50^s{\cdot}8$ L.M.T.

Power 255 adjusted on the star.

Sunday, July 18, 1869.

Occultation of 49 Libræ.

The star disappeared about

$16^h\ 3^m\ 35^s$ L.S.T. = $8^h\ 16^m\ 53^s{\cdot}4$ L.M.T.

This was an unsatisfactory observation.

Power 255 adjusted on the star.

Wednesday, December 8, 1869.

Occultation of δ Capricorni.

The star disappeared instantaneously at the Moon's dark limb at

$22^h\ 45^m\ 34^s{\cdot}9$ L.S.T. = $5^h\ 35^m\ 23^s{\cdot}8$ L.M.T.

There was a great deal of haze.

Power 154 adjusted on the star.

I should state with reference to this observation, that, when I came to look at my slate by lamplight, after I had put down the instant of the star's disappearance, I found that I had, in some

unaccountable way, transposed the minutes and seconds. Had the *minutes* only been right, I should have taken the time of disappearance as 10 seconds later.

Tuesday, December 14, 1869.

Occultation of ξ^2 Ceti.

The star disappeared instantaneously at the Moon's dark limb at

$$2^h\ 55^m\ 45^s{\cdot}3 \text{ L.S.T.} = 9^h\ 21^m\ 26^s{\cdot}7 \text{ L.M.T.}$$

and reappeared at the bright limb, pretty sharply, at

$$3^h\ 48^m\ 28^s{\cdot}6 \text{ L.S.T.} = 10^h\ 14^m\ 1^s{\cdot}3 \text{ L.M.T.}$$

Power 255 adjusted on the star.

Having never yet made an independent determination of the geographical position of my Observatory, I have always employed data afforded by the Ordnance Survey, and assumed it to be situated in Latitude $51^\circ\ 0'\ 58''{\cdot}3$ North and Longitude $17^s{\cdot}5$ East. Our Fellow, Mr. F. C. Penrose, has now most kindly and obligingly computed my longitude from the above disappearance, and finds that, had such longitude depended upon this single observation, "it would have involved an error of only about 1300 yards." I hope, though, to refine upon this considerably.

Forest Lodge, Maresfield, Sussex,
14th January, 1870.

Observations of Jupiter's Satellites, and Occultation of Stars by the Moon. By John Joynson, Esq.

			G.M.T. h m s
15 Nov. 1869	1 Sat.	Ec. R.	7 52 32·0
	3	Tr. I. first	10 9 39
		last	10 38 52
		,, E. first	11 37 7
			11 56 23
22	1	Oc. D. first	7 14 15
		last	7 16 33
		Ec. R.	9 46 31·8
30	2	Ec. R.	5 11 45·0
	1	Tr. E. first	8 23 41
		last	8 28 20
		Sh. E.	8 56 55
			9 1 37
1 Dec.	1	Ec. R.	6 11 29·9

			G.M.T. h m s
3 Dec. 1869	3	Oc. R. first	7 54 10
		mid	7 56 15
		last	7 59 33
		Ec. D.	8 59 58·5
		R.	10 37 31·9
21	3	Tr. E. last cont.	4 58 32
23	2	Shad. full on	5 18 25
	1	Tr. I. first	6 4 7
		last	6 9 14
		E. first	8 9 24
		last	8 16 44
24	1	Ec. R.	6 28 5·5
31	1	Oc. D. first	5 3 47
		last	5 6 12
8 Jan.	2	Ec. R.	7 27 25·1
9	1	Ec. R.	4 49 38·2

Occultations of Stars by the Moon.

			G.M.T. h m s
10 Nov. 1869	30 Capricorni	Disappearance	7 0 30·6
17 ,,	μ Ceti	,,	9 48 19·7
		Reappearance	11 7 28·4

Waterloo, near Liverpool,
12th Jan. 1870.

On a Bright-Cross Micrometer for measuring the position of lines in faint Spectra. By John Browning, Esq.

Attempting recently to map out the spectrum of *Jupiter* I experienced great difficulty in determining the position of the lines. The spectrum was not sufficiently luminous to show the wire of the micrometer when it was opposite the red end of the spectrum, and it was in that portion that I wished to measure the position of the lines discovered by Mr. Huggins, which are probably due to the absorption of light in the planet's atmosphere.

As soon as I tried to light up the wire of the micrometer the spectrum became almost invisible, the lines being altogether lost.*

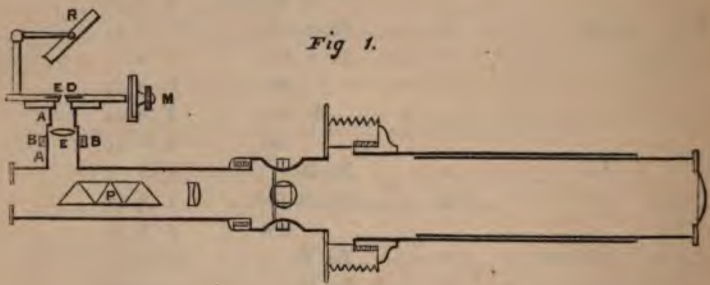
* Is the apparent brightness of *Jupiter* due entirely to the size of his disc? The spectrum of a star of the third magnitude is as brilliant even when widened by a cylindrical lens to the same breadth as *Jupiter's* spectrum.

Since writing the above, Mr. Proctor has kindly furnished me with an answer

To obviate this difficulty I contrived a bright-line micrometer, which is in action, and which I shall now have the honour to describe.

Fig. 1 represents the upper part of the star spectroscope. Attached to the side is a small tube, A A. At the outer part of this tube is a glass plate, blackened with two fine clear white

Fig 1.



lines across the centre, at an angle of 45°, like a letter X. These lines can, perhaps, be most neatly produced by photography.

The lens *c* which is focussed by turning the milled ring M, produces an image of the bright line in the field of view by reflection at the prism from the surface nearest the eye. On turning the micrometer screw, M, the slide which holds the glass-plate is made to travel in grooves, and the fine lines are made to traverse the whole length of the spectrum.

By means of the mirror, R, sunlight can be reflected into the micrometer. If desirable the light may be modified by placing a piece of silver paper spread with paraffin in front of the glass-plate.

Fig. 2 represents a spectrum, and shows the bright cross of

Fig. 2.

the micrometer in measuring a fine line in the most refrangible portion, where a wire or web would be imperceptible.

to this question, I had overlooked the obvious fact that while we have a star in the spectroscope, we obtain the whole light of the disc of the star, between the jaws of the slit, while, in the case of *Jupiter*, we have only the light of that portion of the disc of the planet to deal with which falls within the jaws of the slit. I would, therefore, propose to use the cylindrical lens, such as are used with the star spectroscope, in broadening the stellar spectra, but with the convexity of the lens turned in the other direction, that is to say, in the direction of the line of the slit instead of at right angles to it.

A Method of Constructing Charts by which in a few moments the Great Circle Course between any Two Points on the Globe may be accurately Obtained. By Richard A. Proctor, B.A.

It seems to me almost certain that the plan I am about to describe must have been thought of often before because it is so obvious. But as I have never seen any reference to it in works or papers where such reference was to have been looked for, I venture to bring it before the notice of the Society as having apparently a useful bearing on the problem of great circle-sailing.

It is well known that by Mr. Towson's tables the great circle course from one place to another may be calculated with considerable ease; but a construction by which such a course might be laid down on a chart is, as far as I know, still wanting. I remember having seen a paper (I believe by the Astronomer Royal) in which the difficult problem of laying down great circle courses on Mercator's charts was attacked. Later a short note appeared in which Sir John Lubbock showed how a construction founded on the principles of the gnomonic projection might be applied to the problem. I cannot at present recall where I read those papers, but I have an impression it was in the *Monthly Notices* or else in the *Memoirs* of this Society.

The fact that in the gnomonic projection the great circle course between two points is obtained by simply drawing a line through those points, is inviting. But as one cannot include in a chart even a complete hemisphere on the gnomonic projection, there is an obvious difficulty, which, as far as the purposes in question are concerned, renders the projection almost useless. And, in passing, I may note that so far as I am aware, no one has hitherto pointed out how the course between two points, one in the northern and one in the southern hemisphere, can be deduced (in great part) from gnomonic projections of the greater part of these hemispheres. The solution of the problem is exceedingly simple:—In the Northern map one must draw a straight line through the Northern station and *the antipodes* of the Southern; and the like with the Southern map. These two lines are the projection of the great circle through the points, so far as it falls within the maps. There can be no difficulty in selecting the segments which belong to the great circle course between the points.

The plan I have now to deal with has reference to the stereographic projection. In this projection every circle on the sphere appears as a circle. Every great circle is distinguished by the property that its points of intersection with the great circle around the centre of projection lie on a diameter of that circle. This in effect gives the geometrical construction for laying down a great circle through any two points on the projection. But

practically the way in this can be done on a chart is much simpler.

Every great circle through a place passes through the antipodes of that place. Now, suppose we have a stereographic projection of all the sphere except the south polar region; the north pole being the centre of projection, and we wish to find the great circle course between any two stations:—We find these two stations on the map, and we find the antipodes of *one* station; then a circle carried through these three points is the great circle required.

It would not be necessary to make any construction for determining the centre of the circle through the points. A few moments' trial with a rod-compass would give the circle quite as exactly as the usual construction. I may remark, however, that a useful addition might be made to mathematical instruments, in the form of a ruler by which a cross-line bisecting any given line at right angles might be drawn without any construction. Many forms will suggest themselves. One of the simplest and most convenient is founded on the property that the diagonals of a rhombus bisect each other at right angles. Very likely such instruments are made.

In such a projection as I have spoken of, there would be no occasion to include any regions south of the 50th or 55th parallel of south latitude. Nor need lands and seas be marked in unless it were convenient. If only the meridians (radial lines) and latitude lines (concentric circles) were marked in to every 5th degree (or to every degree, if need were) the deduced great circle course could be transferred in a few seconds to the Mercator's chart, supposed to be similarly divided.

Comet II., 1869.

(Tempel's, see p. 27.) The following elements calculated by M. Leveau from observations at Leipzig, 23 October, and Vienna, 13 and 31 October, are given *Astron. Nach.* No. 1783.

$$T = 1869,\ \text{Oct. } 9,\ 55102,\ \text{Paris M.T.}$$

$$\left.\begin{aligned} \pi &= 139^\circ\ 20'\ 43''\cdot6 \\ \Omega &= 311\ \ 27\ \ 52\cdot0 \\ \iota &= 111\ \ 32\ \ 54\cdot0 \end{aligned}\right\}$$

$$\log q = 0\cdot090056$$

The mean observation is represented as follows,—

$$O - C;\ \Delta\lambda = +0''\cdot5,\ \Delta\beta = +1''\cdot6.$$

Two other observations at Vienna, October 12 and 27, are represented within some seconds of arc.

Comet III., 1869, discovered by M. Tempel at Marseilles, 3 Nov. 1869.

The following elements, calculated by Herr L. Schulhof from observations, Vienna, Nov. 29, Bonn, Dec. 4, and Cracow, Dec. 9, taking account of parallax and aberration, are given *Astron. Nach.* No. 1788.

$$T = 1869, \text{ Nov. } 20, 37996, \text{ Berlin M.T.}$$

$$\left.\begin{array}{l} \pi = 40^\circ\ 37'\ 18''\cdot4 \\ \Omega = 292\ \ 56\ \ 45\cdot4 \\ \iota = \ \ 6\ \ 56\ \ \ 9\cdot9 \end{array}\right\} \text{Mean equinox, } 1870\cdot0.$$

$$\log q = 0\cdot042520.$$

The mean observation is represented as follows,—

$$O-C;\ \Delta\lambda \cos\beta = +7''\cdot1,\ \Delta\beta = +34''\cdot0.$$

Instrument for Sale.

Transit-circle by Jones. The circle 18 inches diameter, divided on silver to 5′, reads with vernier to 3″. Two setting circles divided on silver. Object-glass $3\frac{1}{4}$ inches diameter, by Tully. On a portable iron stand. Apply to Mr. Huggins, Upper Tulse Hill.

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Printed by STRANGEWAYS and WALDEN, Castle St. Leicester Sq. and Published at the Apartments of the Society, Feb. 7, 1870.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXX. *February* 11, 1870. No. 4.

Professor J. C. Adams, Vice-President, in the Chair.

W. A. Harris, Esq., Balliol College, Oxford,
was balloted for and duly elected a Fellow of the Society.

Report of the Council to the Fiftieth General Meeting of the Society.

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
December 31, 1868	182	296	12	3	493	45	538
Since elected ...	6	29	...	...	...	...	...
Deceased	−4	−5	−1	..	...	...	...
Removals	+3	−3	...	...	...	...	...
Resigned	...	−6	...	...	...	...	...
Dec. 31, 1869 ...	187	311	11	3	512	45	557

Mr. Whitbread's Account as Treasurer of the

RECEIPTS.

	£	s.	d.	£	s.
Balance of last year's account	390	14	3		
Error in the Petty Cash Account	0	0	4		
				390	14
By Dividend on £3000 Consols	43	17	6		
By ditto on £5000 New 3 per Cents ...	73	2	6		
By ditto on £3200 Consols	47	0	0		
By ditto on £5000 New 3 per Cents ...	73	8	9		
				237	8
On account of arrears of contributions ...	135	4	0		
155 annual contributions	325	10	0		
32 admission-fees	67	4	0		
28 first years' contributions	42	0	0		
				569	18
10 compositions				210	0
Sale of publications :—					
At the Rooms of the Society	10	2	6		
By Messrs. Williams and Norgate, Publishers	23	16	8		
				33	19
				£1442	c

stronomical Society, from January 1 to December 31, 1869.

EXPENDITURE.

	£ s. d.	£ s. d.
Salaries :—		
Editor of Publications	60 0 0	
Assistant Secretary	130 0 0	
" " Gratuity	20 0 0	
Commission on Collecting	29 0 0	
		239 0 0
Taxes :—		
Land and Assessed	3 5 6	
Income	2 10 0	
Poor Rate	3 10 10	
Other Parish Rates	3 2 6	
		12 8 10
Bills : —		
Strangeways and Walden, printers ...	282 9 8	
Rumfitt, bookbinder	12 19 7	
Basire, engraver	54 10 0	
Pound, "	117 12 0	
Malby and Son, lithographers	98 2 1	
Cundall and Co., photographers	3 8 0	
Insurance	7 15 6	
		576 16 10
Miscellaneous items :—		
House expenses	23 1 9	
Postages	43 16 11	
Books and parcels	3 7 9	
Expenses of evening meetings	13 13 0	
Waiters attending meetings	3 17 0	
Coals and wood	12 0 0	
Gas	6 13 7	
Repairs	2 8 6	
Sundries	12 1 4	
		120 19 10
		949 5 6
Lee Fund	6 0 0	
Turnor Fund	1 6 0	
Mrs. Jackson Gwilt's annuity, 1 year ...	8 15 0	
		16 1 0
Investment :—		
Purchase of £200 Consols	186 0 0	
" " "	187 15 0	
		373 15 0
Banker, Noting Bill		0 1 6
		1339 3 0
Balance at Banker's		102 17 6
		£1442 0 6

ROBT. J. MANN, M.D.
C. G. PRIDEAUX, } *Auditors.*
F. C. PENROSE,

Assets and Present Property of the Society, January 1, 1870:—

	£ s. d.	£ s. d.
Balance at Banker's		102 17 6
1 Contribution of 8 years' standing ...	16 16 0	
8 „ 6 „	100 16 0	
13 „ 5 „	136 10 0	
4 „ 4 „	33 12 0	
19 „ 3 „	119 14 0	
20 „ 2 „	84 0 0	
40 „ 1 „	84 0 0	
Balance of an Account...	2 12 0	
2 admission-fees, and subscriptions ...	13 13 0	
		591 13 0
Due for Publications		2 14 0
£5000 New 3 Per Cents (including Mrs. Jackson's Gift, £300).		
£3400 Consols, including the Lee Fund (£100) and Turnor Fund (£500).		
Unsold Publications of the Society.		
Various astronomical instruments, books, prints, &c.		
Balance of Turnor Fund (included in Treasurer's Account)		139 12 0

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	13	XII.	192	XXV.	199
I. Part 2	53	XIII.	202	XXVI.	204
II. Part 1	71	XIV.	394	XXVII.	459
II. Part 2	35	XV.	176	XXVIII.	419
III. Part 1	87	XVI.	202	XXIX.	451
III. Part 2	106	XVII.	185	XXX.	200
IV. Part 1	105	XVIII.	180	XXXI.	176
IV. Part 2	116	XIX.	187	XXXII.	207
V.	128	XX.	183	XXXIII.	212
VI.	154	XXI. Part 1	216	XXXIV.	200
VII.	178	XXI. Part 2	100	XXXV.	179
VIII.	164	XXI. (together).	94	XXXVI. (with M. N.)	263
IX.	167	XXII.	185	XXXVI. (without)	56
X.	177	XXIII.	180		
XI.	187	XXIV.	187	XXXVII. Part 1	795

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock, No. 1,
The *Beaufoy* clock, No. 2,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle,
The *Brisbane* circle,
The *Baker* universal equatoreal.

The *Sheepshanks'* collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters, with equatorial stand and clock movement.

4. $3\frac{1}{4}$-inch achromatic telescope, with equatorial stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. $2\frac{3}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.

13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff; in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir.

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.

22. Prismatic compass.

23. Mountain barometer.

24. Prismatic compass.

25. 5-inch compass.

26. Dipping needle.

27. Intensity needle.

28. Ditto ditto.

29. Box of magnetic apparatus.

30. Hassler's reflecting circle, with artificial horizon roof.

31. Box sextant and $2\frac{1}{4}$-inch glass plane artificial horizon.

32. Plane speculum artificial horizon and stand.

33. $2\frac{1}{2}$-inch circular level horizon, by Dollond.

34. Artificial horizon roof and trough.

35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.

36. A pentagraph.

37. A noddy.

38. A small Galilean telescope, with the object lens of rock-crystal.

39. Six levels, various.

40. 18-inch celestial globe.

41. Varley stand for telescope.

42. Thermometer.

43. Telescope, with the object-glass of rock crystal.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Fuller* theodolite, to the Director of the Sydney Observatory.

The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument,		No. 1, to Mr. Lassell.
Ditto	ditto	No. 2, to Mr. Huggins.
Ditto	ditto	No. 4, to Rev. C. Lowndes.
Ditto	ditto	No. 5, to Mr. Birt.
Ditto	ditto	No. 6, to Rev. J. Cape.
Ditto	ditto	No. 8, to Rev. C. Pritchard.
Ditto	ditto	No. 9, to the Director of the Sydney Observatory.
Ditto	ditto	No. 41, to Rev. C. Pritchard.
Ditto	ditto	No. 43, to Mr. Huggins.

The 6-inch circular protractor, to Mr. Birt.

The Gold Medal.

The Council have awarded the Gold Medal to M. Delaunay for his *Théorie de la Lune.*

Professor Adams will, in the unavoidable absence of the President, through ill health, explain the grounds of this award.

Printed Transactions of the Society.

Part I. of Volume XXXVII. of the *Memoirs* has been published since the last Report. This Part contains the observations of Col. Tennant on the Solar Eclipse of August 1868. The Part is illustrated by ten plates.

The papers printed in the *Memoirs* of the Society form but a small part of the contributions to the science of Astronomy which have been given to the world through our Society. In the *Monthly Notices*, already in the hands of the Fellows, will be found many papers of interest. In addition to those which form the subject of separate paragraphs of this Report may be mentioned here:—

The Society is indebted for Papers by Prof. Cayley on the attraction of Ellipsoids; on Lambert's theorem of Elliptic motion; and on the determination of a Planet's orbit from three observations.

Mr. Mann's observations of the transit of *Mercury*, Nov. 1868, and Sir T. Maclear's micrometric measures of that planet, which make its observed diameter less than the tabular diameter by $1''\cdot50$. Mr. Proctor has contributed numerous papers to our *Notices;* amongst which, one found in the March Number, on the Transit of *Venus*, 1874, will be especially referred to in another place.

Mr. Balfour Stewart read a short paper suggesting that many auroral appearances, and possibly the zodiacal light, might be caused by convection currents in the upper regions of the Earth's atmosphere, which, being conducted and moving under the lines of the Earth's magnetic force, might be supposed to be vehicles of electric currents.

Mr. Browning, besides observational papers, has described a new form of spectroscope for the telescope, and a method of using an illuminated cross for the measurement of lines in spectra.

Obituary.

The Society has to regret the loss by death of the following Fellows:—

A. K. Barclay, Esq., F.R.S.
Capt. Blake, F.R.S.
Eddowes Bowman, Esq.
E. W. Brayley, Esq., F.R.S.
J. Dickinson, Esq., F.R.S.
Prof. Donkin, F.R.S.
Chas. Mason, Esq.
Benj. Naylor, Esq.
Dr. P. M. Roget, F.R.S.
John Smith, Esq.
Sig. J. Vertu.
Rev. C. Walters.

William Fishburn Donkin was the eldest surviving son of parents remarkable for talent and excellence of character. He was born at Bishop Burton, in the county of York, on the 15th of February, 1814. Very early in life his taste for languages, mathematics, and music, became apparent, and, in addition, he possessed such perseverance and love of acquiring knowledge, that almost before he left the nursery he was known to cry when help was forced upon him which he felt he did not need, and which deprived him of the discovery he wanted to make for himself. The education he received from his father also developed a love for physics and chemistry. In 1829 his family moved to York, in order that he might have the advantage of attending a public school without losing the benefit of home influences. Here, at St. Peter's School, and under the care of Mr., afterwards Archdeacon, Creyke, he made rapid progress in classical learning. To the years spent at this school and directed in his studies by that able master, whom, as a teacher he considered perfect, William always looked back with special pleasure and gratitude.

In October 1832 he matriculated at St. Edmund's Hall, Oxford, and began to reside in the following term. On the installation of the Duke of Wellington as Chancellor of the University he recited the Greek ode. In 1834 he obtained a classical

scholarship at University College; in 1836 a first class in both classics and mathematics; and, in the following year, the Mathematical and Johnson mathematical scholarships, both of which were open to the University.

Soon after this he was elected Fellow of his own College, where he remained for the next six years, holding the post of Mathematical Lecturer, and other College offices. During these years he became known by various publications, among which may be mentioned an "Essay on the Theory of the Combination of Observations," written for the Ashmolean Society, and a very able article on Greek Music published in Dr. Smith's *Dictionary of Antiquities*.

In 1842 he was advised to stand for the Professorship of Astronomy, then vacant by the resignation of Mr. Johnson, since then Dean of Wells; and this he obtained, being at that time 28 years of age. Soon afterwards he became Fellow of the Royal Society, and also of the Royal Astronomical Society. In 1844 he married the third daughter of the Rev. John Hawtrey, at that time Incumbent of St. James's Church, Guernsey.

But even then he was beginning to suffer from a delicacy of constitution, which became a cause of great anxiety to his family; and in the midst of the most hopeful anticipations for science, when his rare abilities seemed to promise some of the discoveries which only fall to the lot of such men as he,—even then he was obliged from time to time to relinquish the duties and occupations in which he delighted, in order to try and preserve in other climates the health which was gradually leaving him. In the intervals of improvement he continued with untiring energy to attend to the duties of his Professorship, and to the advance of science in its various branches in Oxford; and during the years extending from 1850 to 1860 he sent to the Royal Society a few important papers, among which may be named one "On the Equation of Laplace's Functions," and a longer one "On a Class of Differential Equations, including those which occur in Dynamical Problems." Beyond this there is little to say of those long years during which a life that might have been one of the highest distinction was kept in check by illness and consequent inability to work. Those who knew him best, know how brightly the intellect, which otherwise might have enlightened the world, shone in the narrower sphere to which it was now confined; how his failing strength seemed only to enhance the energy that never stopped short of the limits which his state of health set to its achievements, because unable to pass beyond them; the patience that never once complained through all the weariness and comparative obscurity of his life; and the cheerfulness of character and brilliant cleverness which made a centre of happiness to his home and to his friends.

His last work, undertaken towards the close of 1866, was to prepare a book on Acoustics for the Clarendon Press series. For this he was singularly fitted, his acquaintance with practical and

theoretical music being scarcely less extensive and accurate than his knowledge of mathematics; but his increasing illness delayed the work, and the first part was still passing through the press when his friends became aware that he was about to leave them.

His death, on the 15th of November, 1869, cast a shadow over the University of Oxford, which mourned in him one who exhibited that rare combination of the highest scientific powers with the faith of a humble and sincere Christian.

The Rev. C. WALTERS came of a Welsh family, which settled first in the county of Dorset, but, about 200 years since, in Hampshire. His father, the eldest of three sons, was sent to Winchester College, under Dr. Warton (whose good opinion he won and kept), where he left a good name behind him, and made friendships which he maintained, more or less, in after life, with some who subsequently rose to rank and distinction in the University and the Church. His son, the subject of this memoir, was his only child, and he seems to have imbibed, in very early life, a taste for scientific and literary pursuits, his attention having been especially directed towards the former by his uncle, the Rev. John Vodin Walters, whom he ever regarded as a second father, and in whose memory he delighted to the last. He graduated at Oxford about the year 1808, after the usual residence at St. Mary Magdalene Hall. After his ordination he became Curate of Soberton, afterwards of Corhampton and Bishop's Waltham, then Rector of Bramdean, which he resigned for the Rectory of Wyke, near Winchester.

In regard to literary and scientific subjects, the old fire in him never ceased to burn; he gave frequent lectures at scientific institutions, and for many years was President of the Winchester Mechanics' Institute. He was elected a Fellow of this Society in 1838. He died at the advanced age of 85 years, and was buried in Winchester Cemetery.

JOHN DICKINSON, Esq., F.R.S., who died at his residence in Upper Brook Street, on the 11th January, 1869, had nearly completed his 87th year. He had for many years been engaged in the manufacture of paper, many important improvements in which were due to him. His energies were not, however, confined to manufacturing and mechanical details. He took a lively interest in all scientific pursuits, and during his lengthened life was intimate with many distinguished authors and men of science, by whom his varied information, sound sense, and conversational powers, were much appreciated. Besides being a Fellow of this Society he was a member of other learned bodies; in 1845 he was elected a Fellow of the Royal Society, to which he communicated a paper on the supply of water from the chalk stratum in the neighbourhood of London, which contains valuable information as to the amount of the rainfall in Hertfordshire, and the proportion of it which finds its way by percolation to supply the

springs in the chalk. In his latter years he took great interest in astronomy, and erected an observatory at his country house, Abbott's Hill, near Hemel Hempstead. In his astronomical pursuits he was much encouraged by his friendship with the late Admiral Smyth; but though his observatory was furnished with a fine equatoreal and a transit instrument, his advanced age prevented his making much use of them. He was well known as a liberal benefactor to numerous charitable institutions, especially to hospitals, and was for many years one of the registrars of the Royal Literary Fund. His mental and bodily activity were extraordinary, and to use the words of one of his oldest friends,—"He had thoroughly lived out his life, and had found time and means to crowd into short intervals of business more kindly and generous deeds than would make the staple of many ordinary men's lives."

Benjamin Dennison Naylor died on the 27th of December, 1868, after a few days' illness, at his residence The Knoll, near Altrincham, Cheshire. He was the last lineal male descendant of four of the clergy ejected from their livings by the Act of Uniformity in 1662. He was a governor of Chetham's Hospital and Library, Manchester, by right of descent from Humphrey Chetham the founder, who died in 1653; an institution in the management of which he took an active and benevolent interest to the end of his life. He had been many years a Fellow of the Royal Astronomical Society, and was warmly attached to all scientific pursuits, particularly to astronomy and mechanics. He took constant pleasure in his observatory, which he had fitted up with a very fine equatoreal by Cooke of York. Mr. Naylor was also an old member of the Archæological Association.

Eddowes Bowman was born in November 1810. On leaving Hazlewood School, near Birmingham (conducted by the now well-known Messrs. Hill), he entered an iron-foundry, with a view of following the engineering profession. Afterwards he undertook the management of some coal and iron works in Wales, and here the duties imposed on him were fulfilled with scrupulous exactness and to the entire satisfaction of the proprietors. A large portion of his leisure time was devoted to the moral and intellectual improvement of the people with whom he was connected, and by whom he soon became most highly respected. It was here, perhaps, that the peculiar aptitude for teaching developed itself, which characterised him through life.

After a few years thus spent, his tastes took a decided turn in favour of classical literature, and, in order to pursue his studies to greater advantage, he entered Glasgow University, where he took his degree of M.A. He then went to Germany, and studied with great earnestness under some of the most distinguished professors there.

Soon after his return to England, the chair of Greek and

Latin Classics and Greek and Roman History, in Manchester New College, became vacant by the resignation of Mr. F. W. Newman, and Mr. Bowman was then appointed to fill that office, which he did with earnestness, exactness, and thoroughness, until the College was removed to London in 1853.

From this time, without abandoning his favourite classical studies, he seems to have imbibed an increasing taste for natural science, which he pursued in its chief branches till his death, devoting much time and labour to the acquirement of the most recent knowledge in astronomy, optics, heat, electricity, acoustics, &c. These studies he pursued with his accustomed assiduity, not simply from the pleasure he derived from them himself, but that he might be better able to impart the knowledge of them to others; and, to enable him to do this more perfectly, he not only purchased the best apparatus obtainable, but spent much time and ingenuity in constructing numerous diagrams and contrivances for his lecture illustrations. His mode of lecturing was easy and fluent, and his language singularly clear and lucid.

Though astronomy was only one among a number of different branches of science cultivated by Mr. Bowman, yet his deep interest in this particular subject was evinced by the fact that, in a new residence erected some years ago, an observatory formed an important feature, and was constructed expressly to receive a new $7\frac{1}{4}$-inch equatoreal, made for him by Messrs. Cooke and Sons, of York.

It was always a pleasure to him to show this fine instrument to his friends, though his other avocations did not permit him to make so much use of it himself as either he or others could have wished.

Mr. Bowman was elected a Fellow of this Society in 1864. He died July 10, 1869.

As Signor VERTU was little known beyond the circle of his professional duties, a few words of information concerning him may prove interesting. He was born in the valleys of the Vaudois in Piedmont, and was educated at the university of Lausanne. It was there, while under the tuition of the celebrated mathematician, Emman Develey, that he began to take an interest in Astronomy; he became very fond of the science, and was possessed of some good instruments, but his time being almost absorbed by his professional duties, he sent but one contribution to the Society, which is to be found in Vol. xxiii. of the *Monthly Notices.* He died at his residence, 40 Cromston Street, Derby, on the 22nd of July, 1869, after a short illness.

Proceedings of various Observatories.

Royal Observatory, Greenwich.

The chief special work which has occupied the attention of the Astronomical Department of the Royal Observatory during the past year has been the preparation of the new Seven-year Catalogue of Stars for 1864, good progress of which has already been reported. The calculations are now completely finished, and about one-third of the Catalogue is printed; the whole work being intended to form an Appendix to the *Greenwich Observations* for 1868. We believe that its publication will be a great boon to all practical astronomers, especially as the Catalogue contains a large number of stars which have not before been observed at Greenwich since the time of Bradley.

Although the preparation and the reading of the proof-sheets of the new Seven-year Catalogue have formed the chief additional work of the Royal Observatory during the past year, absorbing necessarily a considerable portion of the computing strength; yet the ordinary observations and computations have not suffered seriously. The Sun, Moon, the principal planets, the minor planets in the first half of the lunation, the necessary stars for the determination of the clock and instrumental errors, with others whose places are required for special purposes, have all been observed with the Transit-Circle with the utmost regularity. Observations of the Moon, and of the stars and collimator, have also been daily made with the altazimuth when practicable.

The printing of the astronomical part of the volume for 1868 is in a very forward state, and the reduction of the observations made in 1869 is so far advanced that a considerable portion of the results is nearly ready for the printer.

The volume for 1868 will contain three Appendices:—1. Mr. Breen's paper, "On the Corrections of Bouvard's Elements of the Orbits of *Jupiter* and *Saturn*." 2. The Greenwich Seven-year Catalogue for 1864. 3. A Description, with Plans, of the Great Equatoreal.

Royal Observatory, Edinburgh.

The observations of fifty-five stations of the Meteorological Society of Scotland have been computed here during the last year, and published by the Registrar-General in Scotland. Time-signals by gun and ball have been daily given. Meridian observations of stars with the Transit Instrument and Mural Circle have been carried on, but not to the full extent, on account of the reduction of past observations having been taken up. A

Report to the Board of Visitors appointed by Government was published last May, and refers to several matters of exceptional interest.

Radcliffe Observatory, Oxford.

The principal change during the past year to be noted with regard to this Observatory is in its personal staff, arising from the death of the first assistant, Mr. A. Quirling. Mr. Quirling had been suffering from severe illness for several months before his death, which occurred on the morning of the 8th of June last. The vacancy which occurred in the establishment on this account, has been filled up by the appointment of Mr. Lucas to be the principal assistant, and by the engagement of Mr. S. Béchaux, B.A. of Sidney Sussex College, Cambridge.

Mr. Béchaux took first-class mathematical honours in the year 1861, and the energy and zeal with which he has devoted himself to the active duties of astronomy since his connexion with the observatory, has been already productive of the happiest effects.

The illness and death of Mr. Quirling of course produced some derangement in the business of the establishment, but not to so great an amount as might have been anticipated, as will be shown by the work which has been accomplished during the last year.

It may be mentioned, in the first place, that the volume of the Radcliffe observations for 1866, was published last midsummer, and that the astronomical portion of the volume for 1867 is nearly completed, some copies of the Catalogue of Stars (1477 in number) for that year having been circulated amongst astronomers.

The reductions of the observations for 1868 are nearly completed. The meridional observations for that year include a catalogue of about 1700 stars, 114 observations of the Sun, 63 of the Moon, 27 of *Mercury*, and 18 of *Venus*. Seven occultations of stars by the Moon were made and reduced, and the heliometer was used when the weather permitted for the observations of double stars.

For the year 1869, the transits are thoroughly reduced and ready to be made into ledgers for the formation of a catalogue of stars, in number about 1400, but only the first steps of the reduction of the North Polar distances have been performed. The number of meridional observations made during the year is quite equal to the average, the number of transits being about 3000, and of zenith distances about 4000. The usual quantity of extra-meridional work was also done in the year.

The photographic meteorology has been carried on by Mr. Lucas with the same success as heretofore, and the reductions for 1867 are nearly completed.

But the most remarkable work which has been completed during the year is the Second Radcliffe Catalogue of Stars which has very recently been finished, and some copies distributed amongst astronomers. As this Catalogue will have a separate notice in the general review of Astronomy for the past year, it will be sufficient in this place to say that it contains 2386 stars observed between the years 1854 and 1861, both inclusive.

Cambridge Observatory.

The meridian observations with the transit instrument and mural circle were made in the usual order until July 10th, when the former instrument was dismounted in preparation for the reception of the new Transit-Circle. These were chiefly confined to observations of standard stars for the determination of clock and instrumental errors, of *Mars* near opposition, and of the Sun near the equinoxes and solstices. Since July 10th, the time has been approximately found by transits taken with the telescope of the mural circle. Winnecke's comet was observed on thirteen separate nights in May and June with the Northumberland equatoreal; on each night it was compared with several stars carefully selected from catalogues. The results are found to be very accordant. Both the equatoreals have been used in observing occultations.

The meteorological observations have been made at the usual hours of 9 A.M. and 3 P.M.

The courses of a few meteors were registered in August, but the sky was unfavourable in November.

Steady progress has been made during the year with the calculations and preparations for the press; the observations of more than 1000 stars, which were made in 1864 and 1865 with the Northumberland equatoreal, by means of the square bar micrometer, for the purpose of completing the Markree zones, have been reduced, and the mean places obtained for the beginning of 1865.

The observations of Winnecke's Comet have been reduced, and the comet observations of former years are now being arranged for the press. The transit reductions are completed. The mural circle reductions are completed to the end of 1866.

All the preparations have been made for some time past for the new Transit-Circle, the arrival of which has been most unaccountably delayed, as in October last it was expected.

Glasgow Observatory.

The operations at the Glasgow Observatory during the past year have been essentially the same as those of preceding years. In addition to stars chosen from the usual list, several of the minor planets have been observed with the transit circle. The Ochtertyre equatoreal has been employed in observing such of the minor planets as may be conveniently seen with it, and do not come within the range of the meridian instrument. A few observations of Brorsen's and Winnecke's comets have also been obtained with the same instrument.

True time continues to be transmitted in the usual manner to the city and port of Glasgow. A system of meteorological observations, conducted by means of self-recording instruments, was established at the observatory in the beginning of 1868 under the auspices of the Meteorological Committee of the Royal Society, and has continued since in active operation.

Work done with the Photoheliograph at the Kew Observatory.

The first instalment of the measurements and reductions of the Kew Sun-pictures taken during the two years 1862 and 1863, containing also the areas of the observed groups and an explanation of the methods followed in the working out of the observations, has been published in the last volume of the *Transactions of the Royal Society*. Nearly 150 separate copies of the paper, printed partly at the private expense of Mr. Warren De La Rue, were distributed, chiefly to foreign observatories, scientific institutions, and distinguished astronomers and physicists.

The second instalment, containing the heliographic positions of the Sun-spots observed from the beginning of 1864 to the end of 1866, is nearly ready, and will be presented to the Royal Society at an early date during the present session.

Some investigations were also made last year on the influence which a refracting medium of considerable density would have on the apparent size and figure of the Sun, and the time of rotation, as calculated from spots at different latitudes. The preliminary discussion has led to the conviction that a comparison of the times of rotation, as derived from spots while they are near the limb, with those deduced from the same spots when near the centre, will throw much light on several important questions connected with solar physics. The matter will be exhaustively investigated in the general discussion of the Kew results.

During the present year it is intended to bring, if possible, the work of the measurements up to date. The scarcity of spots during 1867 and 1868 was such, that the pictures of these years

may be measured in a comparatively short time; and it is hoped that, by completing, by the end of this year, the observations made up to at least the end of 1869, that the greater part of the succeeding years may be devoted to as careful a discussion of the whole work as is required by the importance of the astronomical and physical problems involved in it.

Messrs. De La Rue, Stewart, and Loewy, state that the reductions of Hofrath Schwabe's observations are now finished. By comparing his observations with those taken by Carrington, and also with the Sun-pictures taken at Kew, they arrived at very favourable conclusions regarding the accuracy of the delineations of the distinguished German observer. Beginning with the year 1832, they have measured the spotted area of all his pictures up to the time when Carrington's series commenced. From the results obtained they have, first of all, deduced fortnightly views, and in the next place, in order to get rid of the more transitory fluctuations, they have taken a series of those monthly views corresponding to the middle and end of each month, from the beginning of 1832 till the end of May 1868. Putting these results into a graphical form, they have obtained a curve exhibiting only the irregularities of comparatively long periods, and from the curve by the ordinary method of equalisation, they have deduced an equalised curve exhibiting the decimal period of solar disturbances.

They find therefrom the following epochs of maximum and minimum spotted area:—

Minimum	Nov. 28	1833
Maximum	Dec. 21	1836
Minimum	Sept. 21	1843
Maximum	Nov. 14	1847
Minimum	April 21	1856
Maximum	Oct. 7	1859
Minimum	Feb. 14	1867

From these dates it will be perceived that (as has been already observed) the time between the minimum and maximum is always less than that between the maximum and next minimum.

It will also be noticed that the whole period is not always of uniform length; nevertheless, judging from what has gone before, they believe that they are perhaps entitled to conclude that the approaching minimum will not be delayed much beyond the end of this year. It ought also to be remarked that, in all the three series, the progression from maximum to minimum is not a simple progression, but exhibits in each case traces of a secondary maximum.

Finally, they have examined these results for traces of the action of the planets upon Sun-spots, and pursuing the method indicated in their preliminary researches, they derived the fol-

lowing table, as exhibiting the evidence deduced from all the observations between 1832 and 1868:—

Relative Planetary Separation.			Excess or Deficiency of Spotted Area. Jupiter and Venus	Mars and Mercury.
Between 0° and		30°	+ 881	+1675
30	,,	60	− 60	− 139
60	,,	90	− 452	−1665
90	,,	120	− 579	−2355
120	,,	150	− 705	−2318
150	,,	280	− 759	−1604
180	,,	210	− 893	− 481
210	,,	240	− 752	+ 547
240	,,	270	− 263	+ 431
270	,,	300	+ 70	+ 228
300	,,	330	+ 480	+1318
330	,,	0	+1134	+2283

From this table there appears to be an excess of solar activity when either *Jupiter* and *Venus* or *Mars* and *Mercury* are together, and a deficiency when they are 180° apart. We see also that the progression of the numbers is regular in each case and very similar in the one case to what it is in the other.

Stonyhurst College Observatory.

The principal building of this Observatory stands on slightly elevated ground in the centre of the college garden. It was built in 1838 for astronomical and meteorological observations.

The ground-floor consists of a central octagon, 22 feet across, with four transepts, each 10 feet by 8 feet, at the four points of the compass. Over the centre of the octagon is a circular room, surmounted by a revolving cylindrical roof, 10 feet in diameter. This upper room was built for an achromatic by Jones, equatoreally mounted, of 4-inch clear aperture, and 5 feet 6 inches focal length. A meridian circle, by the same optician, was placed in the east transept, and another transit occupied the west transept. The diameter of Jones's meridian circle is 2 feet 6 inches; the telescope having an object-glass of 3 feet 6 inches focal length, and 3 inches aperture. Two sidereal clocks were also procured for the Observatory.

In October, 1850, the Rev. A. Weld determined very accurately the longitude of Stonyhurst by means of three of Shepherd's chronometers, the old observatory at Liverpool being chosen as the station of comparison.

Longitude of Stonyhurst	$9^{m}\ 52^{s}\cdot68$ W. of Greenwich.
Latitude of Stonyhurst	53° 50′ 40″ N.

In 1867 the Observatory underwent a considerable change. The 8-inch Equatoreal, which formerly belonged to Mr. Peters, was purchased by the college authorities, and a special building erected for its reception. This new observatory was placed at a considerable distance (75 yards) from the old one, for fear lest otherwise the massive iron pier on which it rests might disturb the magnets suspended in the subterranean portion of the principal building.

A circular room, 24 feet in diameter, with a revolving dome, a transit-room of 10 feet square, containing an instrument by Cary of 3 feet focal length and $2\frac{3}{4}$ inches aperture, and a study 10 feet by 12, to be devoted to spectroscopic researches, form the whole of the new observatory. The foundations of the pile for the equatoreal have been constructed with the greatest care. A bed of concrete, 10 feet square and 2 feet thick, rests on a natural foundation of coarse growing sand. On the concrete are placed four dressed stones side by side, forming a square, and each weighing more than 10 cwt. These four stones support an hexagonal column, 5 feet 6 inches in diameter, each layer of which is composed of six large cut stones, and the whole is finished with a Yorkshire landing 4 inches thick. The total height from the gravel bed to the levelling screws of the instrument is about 17 feet. Underneath the floors of the building are ceiled cellars in which a hot-water apparatus has been fitted up for protection in damp weather. The support of the telescope is an iron pier cast in two pieces; viz. a foot which weighs 13 cwt., and a pillar weighing 17 cwt. The two are fastened together by iron nuts, which allow a little play in azimuth, and the foot rests on eleven blunt levelling screws.

The object-glass, made by Troughton and Simms, has a clear aperture of 8 inches, and 11 feet 6 inches focal length. The diameter of the N.P.D. circle is 2 feet 2 inches, whilst those of the R.A. and hour-angle are each 1 foot 7 inches. The engineer who mounted the telescope was Napier, but the fixings of the N.P.D. circle are by Cary. The clockwork, which is perhaps the most perfect portion of the instrument, requires winding only every six hours. It goes excellently, and has no strain upon it, since it serves only to regulate the motion, the motive power being a weight suspended within the iron pier.

The telescope is well provided with eye-pieces and micrometers; the highest power is about 750.

The dome, which covers the instrument, runs on twelve 6-inch rollers working freely between an upper and lower surface of railway metal. The flange of the upper railway metal is bolted to the sole plate of the dome. The framework of the dome is of pitch-pine, and out of the sole plate circular ribs converge to the zenith. The frame is stiffened with a reticulation of hoop-iron, and covered with $\frac{1}{2}$-inch boards of pitch-pine. The outside is protected by a covering of painted canvass, and the internal finish is of American cloth of a dark colour. The whole

is moved by a spur wheel and pinion, gearing into a segmental rack, which rests on the wall plate. The wheel is actuated by a chain. This is so placed as to be directly opposite the shutter, and therefore generally near the observer's hand. The shutter consists merely of a frame of pitch-pine, covered outside with painted canvass, and inside with American cloth. It is worked by pulleys and bevel gearing, and its width is 4 feet 6 inches.

During the past year considerable advance has been made towards rendering the observatory practically complete. A stone pier has been erected on a brick foundation in the open air, and an equatoreal mounting attached to it. This mounting can be used for any tube, but it is principally intended for a $7\frac{1}{2}$-inch Newtonian, with which the heavens can be freely swept without the necessity of moving a large dome. The two observatories have been connected by an underground wire to avoid the necessity of daily observing clock stars with two transit instruments and to have a sure check on the time at either observatory. A direct vision spectroscope has been adapted to the equatoreal by Browning. In the summer six compound prisms were procured from Hoffman of Paris, and these are being mounted for a spectroscope by Troughton and Simms. A hot-water apparatus has been arranged for the protection of the equatoreal dome and the adjacent rooms. Two observing-chairs have also been made for the large telescope. And, lastly, a new 8-inch object-glass has been ordered from Dallmeyer, the present glass not being considered sufficiently good for the excellent mounting of the instrument.

The work done has so far been mostly of a tentative character. Observations of the Moon's surface, measures of the diameters of planets and of the distances between double stars, Winnecke's comet, the November meteors, and some preliminary work with a direct-vision spectroscope, constitute the miscellaneous observations of the last twelve months. Correct time for the magnetic and meteorological self-recording instruments being a want continually felt, no fine night is allowed to pass without clock-stars being observed.

The meteorological observations, made with instruments that have been carefully compared at Greenwich or at Kew, extend in an uninterrupted series over twenty-three years.

In 1866, Stonyhurst was chosen as one of their three English meteorological stations by the Board of Trade, and was provided accordingly with self-registering barometer, thermometers, and anemometer. These instruments arrived in December 1867, and hourly readings, as also the maxima and minima of the curves traced by them, have been taken here since January 1, 1868. Both the curves and the readings are forwarded weekly to the Central Observatory at Kew.

A monthly summary of the results obtained with the meteorological instruments has been printed for private circulation during the past ten years, and a table of the reduced monthly

magnetic observations is now added to the yearly meteorological means.

The magnetic department is at present the most complete portion of the observatory. A continued series of monthly determinations of the dip, horizontal force, and declination, extend over somewhat more than six years. These have been reduced this year for the purpose of calculating the annual inequalities, &c. Previous to 1863, when a small chronometer was replaced by Frodsham's No. 3148, only occasional observations were made with the dip circle, No. 32 of Barrow, and Jones's unifilar, purchased in July, 1858; but these scattered results are of considerable use for finding the secular variation of the magnetic elements. A suitable building in a retired part of the college garden has been erected for these instruments.

During the long vacations of 1868 and 1869, a magnetic survey of the whole of France has been made with the Stonyhurst magnetic instruments, and at the expense of the college authorities. The results of the survey of the west of France, made in 1868, were presented to the Royal Society in June last, and the more numerous series of observations taken in 1869, in the east of France, are at present in process of reduction.

In 1866 a grant was made by the Royal Society towards the expense of a set of self-recording magnetographs for this observatory. In consequence, two subterranean chambers were built adjoining the old observatory. One is a photographic room 15 feet by 12 feet. The other, a chamber 20 feet by 18 feet, in the centre of which stand the pillars supporting the three magnetographs, with two additional pillars holding telescopes, by which the observer is able to read, at any moment, the state of the magnets without interfering with the continuous photographic record. This last room is well guarded against all damp or variation of temperature. The roof is arched, with two rings of brick set in blue lias lime, and the whole covered with 6-lb. lead, and then with earth and gravel. The walls are three in number. The inner one is a single brick wall set in hydraulic lime. Surrounding this is a cavity for air 3 inches wide, with air-holes 1 foot apart, all round the room. Enclosing this is a rubble wall, 2 feet thick. And at the outside of all, as a protection from the surrounding earth, is 1 foot of loose rubble, which serves admirably for drainage. The flagged floor is built upon piers, and is thus raised 18 inches above the sand. Owing to these precautions the room keeps remarkably dry, and the temperature may be considered almost constant.

Continuous photographic records have been obtained of the declination, and of the two components, horizontal and vertical, of the intensity since January, 1868. The curves traced during the magnetic storms, which were coincident with the grand auroral displays of April and May last, were of the most perfect description, a disturbance of more than $2^{\circ}\cdot5$ in 9 minutes, having been clearly recorded in the Declination Magnetogram. Some

of the curves have been compared with those of Kew, and also with those of Signor Donati of Florence, with very interesting results.

Mr. De La Rue's Observatory.

During the past summer and autumn, numerous experiments have been made to render the luminous prominences of the Sun visible, when that luminary is not totally eclipsed. In order to effect this object, films of gold and other metals have been interposed between the 13-inch mirror and the eye-piece; and fluid coloured media have been also employed for the same object. These experiments will be again resumed during the present year.

An investigation is also being carried on, having for its object the production of a true parabolic figure, with greater certainty than has been obtainable by any of the methods hitherto used in making specula for reflecting telescopes.

Mr. Lassell's Observatory.

Mr. Lassell remarks that since his return from Malta, he has almost entirely neglected astronomy; but he has at length re-erected his two-foot equatoreal, nearly as it was at Starfield, and at Bradstones. He has chosen the form of a drum-dome, as possessing greater permanence of shape; and therefore affording greater facility of opening and shutting, without danger of the shutters being set fast by increased or unequal friction by warping. Mr. Lassell thinks that the greater capacity of the building is also advantageous, by its securing a more uniform temperature. The dome revolves on six eight-inch cannon-balls, working between cast-iron channels, the sectional curvature of which is six inches radius. The lower channel is screwed to a ring consisting of two strata of inch and a half deal plank, eleven inches broad, cut into lengths of three feet six inches, and bolted together with the end joints alternating. This ring is secured by half-inch bolts, built in the brickwork, to a nine-inch circular wall, whose interior radius is fourteen feet six inches. The upper channel is screwed to the under surface of a similarly constructed ring, of pieces of plank *three* inches thick. This forms the base ring of the dome, on which are supported three other lighter rings by intervening crosses, these lighter rings and crosses being respectively made out of inch, and inch and a quarter plank. Across the circle of the uppermost ring, but two feet six inches from its centre, is laid a trussed beam, twenty-nine feet eight inches long, which carries the side-supports of the horizontal shutter, distant four feet eight inches, and extending quite across the building. This shutter, about eighteen feet long, travels on rollers between these supports, and when withdrawn, exposes the vertical sky from the circumference of the dome to two feet six inches beyond the

centre. The roof, principally borne by the trussed beam, and having no more inclination than is necessary for carrying off the rain, is covered (as well as the circumference of the dome) with three-eighth inch deal board in two strata, which finally received a coat of well-painted canvas. The vertical shutter is framed of wood to the radius of the building, travelling on two large pulleys guided by a rail, and when opened, slides within that part of the dome which is adjacent to the opening on the preceding side, so as to be completely out of the way, and unaffected by wind, however violent.

The mode of carrying on the dome while observing, is by attaching a light pair of three-sheave blocks to the wall-plate and base-ring, the end of the rope, or fall being carried to a hook or belaying-pin attached to the observing staircase. The facilities for opening and closing the dome are by this construction eminently satisfactory, either operation being performed *in less than one minute,* without any irksome exercise of muscular power.

Mr. Lassell has also introduced a slight variation in the *mounting* of the telescope, having substituted a cast-iron frame for the mass of masonry which formerly supported the upper part of the polar axis; which renders the instrument more capable of removal, should that ever be required.

He is glad to say that the condition of the telescope on this remounting gives him the greatest satisfaction. The speculum has been laid up under a coat of varnish for more than eight years; yet on this being carefully dissolved off with a little absolute alcohol, the surface reappears with no sensible deterioration of either lustre or defining power; and Mr. Lassell sees minute points of light and the surface of *Jupiter* (barring a little abatement of his own visual acuteness) just as well as ever. He is inclined to think this size of telescope the very best in point of efficiency that can be, since it is just within the power of one observer to manage it, without being dependent on an assistant.

Mr. Lassell is afraid that he cannot promise for the future to be a very active observer; and during the few opportunities he has yet had, he has been most struck with the phase of *Jupiter*, which presents this year an aspect more distinguished by a profusion of narrow sharp belts, together with on the whole a more uniform aspect from day to day and from week to week, than used formerly to be present. On reference to a pretty numerous series of sketches of the planet which Mr. Lassell has made during more than twenty years, he is struck with the variety of configuration which has presented itself, without precisely the same form having hitherto recurred. When we reflect upon the magnitude of these colossal changes on the surface of a planet of so vast a size, we cannot help being impressed with a feeling of intense admiration, and of curiosity (if that were of any avail) to know how they are produced.

Mr. Huggins' Observatory.

In the autumn of 1868, numerous experiments were made for the purpose of finding a method by which the forms of the solar prominences could be observed directly. Several new arrangements of the spectroscope were constructed with this object in view, but at last the very simple method of employing a wide slit suggested itself, and on Feb. 13, Mr. Huggins succeeded in this way in observing *directly*, and not by inference, from the varying length of the bright lines, the outline of a solar prominence. When a spectroscope of only moderate power is employed, Mr. Huggins found it to be of advantage to limit the field of view of the little telescope to the part of the spectrum where the light of the prominence is situated, and further to interpose a red glass when the light is found to be inconveniently bright to the eye.* Other experiments were carried on for the purpose of rendering the solar prominences visible without a spectroscope, by means of the property of selective absorption possessed by many-coloured media, a method which presented itself to Mr. Huggins three or four years since. The problem to be solved was to find a combination of media which would absorb light of all refrangibilities except precisely that of C., or of F. A large number of mineral and vegetable substances was examined; the most promising combination which has been found consists of a solution of carmine in ammonia which cuts off nearly all the light more refrangible than C., combined with a solution of chlorophyll which gives a band of absorption, taking away the brighter part of the light less refrangible than C. As yet chlorophyll has only been obtained in a state in which the band of absorption encroaches upon C., and weakens the light of the prominence.†

The observations in this observatory were interrupted in the autumn by the necessary preparations to fit the building for the reception of a refractor of 15 inches aperture, which is being constructed by Mr. Grubb of Dublin, for the Royal Society, by whom it will be placed in the hands of Mr. Huggins.

* *Proceedings Royal Society*, vol. xxvii. p. 302.
† *Monthly Notices*, vol. xxx. p. 36.

Notes on some Points connected with the Progress of Astronomy during the Past Year.

Discovery of Minor Planets and Comets.

In the last Annual Report a record was made of the discovery of the unprecedented number of twelve asteroids during the year 1868. In this respect, the planetary additions in 1869 contrast very unfavourably with those of the preceding year. The general cloudy state of the sky during the last twelve months has interfered sensibly with delicate astronomical observations of all kinds, and none more so than with those of the minor planets and similar faint objects. However, during occasional intervals of clear weather, two of these minute bodies have been added to the asteroid list; (108) *Hecuba*, discovered on the 2nd of April 1869, by Dr. R. Luther, at Bilk; and (109) *Felicitas*, on the 9th of October, by Dr. C. H. F. Peters, at Hamilton College, Clinton, New York.

Several of the minor planets discovered in 1868 have received their distinctive names since the date of the last Report, a record of which may be useful in this place. They are (99) *Dike*, (103) *Hera*, (104) *Clymene*, (105) *Artemis*, and (106) *Dione*.

Three telescopic comets have been detected during the past year. The most important is Comet I. 1869, known also as Winnecke's periodical comet. This object had been identified at a previous perihelion passage with a comet discovered by Pons in 1819. According to a recent paper by M. Oppolzer, inserted in the *Astronomische Nachrichten*, we gather that there are very strong grounds for believing that it is also identical with a comet discovered by M. Pons so far back as 1808. M. Oppolzer, however, hopes to be able to speak more definitely on this point when he has completed some additional investigations on the comet's orbit, on which he is now engaged.

The two remaining comets—Comet II. 1869, and Comet III. 1869—were both discovered by M. Tempel, of Marseilles, the first on October 11, and the second on November 27. Several observations have been made of each at different observatories and approximate elements of their orbits have been computed.

Proposed Prizes for the Discovery of Comets.

The Fellows will be glad to learn that the *Imperial Academy of Sciences at Vienna*, acknowledging the importance of continuous cometary investigations, especially in connexion with

observed streams of meteors, has proposed to give a series of prizes, limited to eight annually, for the discovery of telescopic comets during the three years' interval between May 31, 1869, and May 31, 1872. The prizes will be awarded at the annual meeting of the Academy, which usually takes place at the end of May, and will consist either of a sum of money,—twenty Austrian ducats—or of a gold medal of equivalent value. In a circular distributed among astronomers, the Academy has published a series of rules for the guidance of the discoverers. From its principal clauses, it appears that the comet must be invisible to the naked eye at the time of discovery, and also one whose appearance could not have been predicted with any degree of certainty. Notice of the discovery must be immediately sent to the Academy at Vienna either by post or telegraph, without waiting for further observations. The message must, however, always be transmitted by telegraph when practicable. The authorities of the Academy undertake to communicate the discovery immediately to the principal observatories. The prize will not be awarded to any discoverer of a comet which has not been observed by another astronomer unless his first record of the comet's place is supplemented by further observations, sufficient in number for the determination of the elements of the orbit.

The Council are glad to hear that the *Imperial Academy of Sciences at Vienna* has thus so generously determined to encourage a systematical searching for telescopic comets, a branch of astronomical discovery which has been comparatively neglected of late years. They trust that the initiative taken by the Vienna Academy will be heartily responded to, not only by the Continental astronomers, but by the Fellows of this Society, many of whom are now so well furnished with the necessary instruments for cometary observations.

Division of Labour in Computing Ephemerides of the Minor Planets.

Observers of the minor planets are now practically dependent upon the *Berliner Jahrbuch* for the annual ephemerides of these objects. The rapid increase in their number renders it necessary to make a distribution of labour among computers, to allow of proper provision being made for the computation of the tabular places. The Fellows may, therefore, be interested to know by whom the important work which appears in the pages of the *Berliner Jahrbuch* is accomplished. The following list gives the names of the computers, and the planets for whose tabular places for 1870 each is responsible :—

M. Adolf	Helena, Mnemosyne.
Dr. Albrecht	Sappho.
Dr. Anderson	Undina.

Dr. Auwers	Circe.
Dr. Von Asten	Cybele, Diana, Egeria.
Dr. Becker	Amphitrite, Beatrix, Clotho, Erato, Eugenia, Europa, Hygeia, Niobe.
Dr. Bruhns	Bellona, Irene.
Dr. Brunn	Isis.
Dr. Celoria	Clytie.
Dr. Deike	Thisbe.
Dr. Dunér	Panopea.
Dr. Engelmann	Eurydice.
Dr. Gunther	Ægina, Calliope, Calypso, Daphne, Euterpe, Galatea, Lætitia, Massilia, Phocea, Urania.
Mr. Hall	Terpsichore.
M. Hoek	Proserpine.
M. Karlinsky	Hestia.
Dr. Kowalczyk	Hesperia.
Dr. Krüger	Themis.
M. Lehmann	Eunomia, Metis, Minerva, Vesta, Victoria.
M. Leppig	Aurora, Flora.
M. Leveau	Hera.
M. Liegel	Lutetia, Pomona.
Dr. Lorek	Semele.
Dr. R. Luther	Danaë, Hebe, Melete, Parthenope.
M. Möller	Pandora.
Nautical Almanac	Pallas
Dr. Oppolzer	Angelina, Concordia, Olympia.
Dr. Peters (Altona)	Sylvia.
Dr. Peters (Clinton)	Echo, Feronia, Frigga, Ianthe, Io, Miriam.
Dr. Powalky	Aglaia, Doris, Fortuna, Freia, Harmonia, Nysa, Pales, Virginia.
M. Prey	Ariadne.
Mr. Safford	Alcmene.
Dr. Schultz	Alexandra.
Dr. Schur	Arethusa, Melpomene.
M. Sievers	Atalanta, Leucothea, Polyhymnia, Psyche, Thalia.
M. Stark	Hecate.
Dr. Stolz	Asia.
Dr. Tiele	Fides.
Dr. Tietjen	Ausonia, Leda, Nemausa.
Dr. Tischler	Eurynome.
Dr. Valentiner	Clio.
M. Vogel	Ægle, Antiope.
M. Westphal	Euphrosyne.
M. Wolfers	Ceres.
M. Wolff	Julia, Leto.

The Total Solar Eclipse of August, 1869.

This eclipse was successfully photographed in America, by two of the eclipse parties organised, on the proposal of Prof. Coffin, superintendent of the *Nautical Almanac* of the United States, by Prof. H. Morton, of Philadelphia, who took charge of these expeditions. It was also photographed by Commander Ashe, director of the observatory at Quebec.

The apparatus employed by the American astronomers was similar in principle, but of larger optical power, than that used by De La Rue in Spain, in 1860, that is to say, the principal image was enlarged by a Huyghenian eye-piece before it reached the sensitive plates. The adoption of this form of apparatus was decided upon "after a careful study of De La Rue's reports and pictures, as also those of the late expeditions," which led Prof. Morton to the conclusion that the plan of enlargement presented many advantages over that of making the photograph directly in the principal focus of the telescope; one of these advantages consists in the superior definition of the position-wires over that which obtains when they are situated within the focus of the telescope, as in the form of instrument used by Major Tennant. Certain modifications in the form of the Huyghenian eye-piece were made by Mr. Zentmayer, in order to adapt it specially for the photographic work, and the instantaneous slide was also somewhat different from that used by De La Rue. The telescopes employed in taking the photographs possessed a great advantage in respect of aperture over the Kew Heliograph, being 6 inches in diameter, whereas the object-glass of the Kew instrument is only 3·4 inches in diameter; moreover, in the instruments used in the American expeditions, the primary image of the Sun is enlarged only to about two inches in diameter, whereas in the Kew Heliograph the image is enlarged to 3·8 inches, so that during totality, when the full aperture of the objective was employed as in both cases in the Kew Heliograph, the area-ratio of the objective to that of the picture was as 0·8 to 1, whereas in the American telescope it was as 9 to 1, or about 11·25 times greater. Six pictures were taken during the totality at Burlington, $40^{\circ}\ 48'\ 17''$ N.L., and $0^{h}\ 56^{m}\ 14^{s}$ west of Washington; and four at Ottumwa, situated about 75 miles west of Burlington, by another party. The totality pictures at Burlington were obtained with exposures of from 5 to 7 seconds, and those at Ottumwa in from to 6 to 16 seconds. In 1860, De La Rue's first picture was obtained with an exposure of 60 seconds; on taking into consideration the relation of aperture to size of picture, the Burlington pictures would represent an exposure of 56·25 to 78·75 seconds, and the Ottumwa 67·5 to 180 seconds in the Kew Heliograph. It does not, therefore, appear that any step has been made in the sensitiveness of

the chemicals since 1860, or that the luminosity of the protuberances greatly differed in 1869 from that in 1860.

The American pictures are very perfect specimens of astronomical photography; and have the advantage from the relative shortness of the exposure of showing the state of the phenomena in reference to a given epoch with more precision than was possible in 1860. By comparing together the photographs obtained at Burlington and Ottumwa, it is seen that the protuberances are absolutely identical in both series except in so far as they are more or less covered in consequence of the parallactic displacement of the lunar disc. They also show that those brilliant surroundings of the Sun, not bounded by such distinct outlines as the protuberances, and yet whose forms are clearly distinguishable, are identical in both series of the photographs. This goes to confirm the photographic observations of De La Rue in 1860, and Tennant and Vogel in 1868, showing that the protuberances properly so called do not constitute the sole surroundings of the Sun's photosphere, but that a portion at least of the coronal brightness belongs also to the Sun. On the occasion of this eclipse, it was noticed by Prof. Pickering, who accompanied one of Prof. Morton's parties to Mount Pleasant, that, while "the sky was strongly polarised all around, close up to the corona, that object itself was not a source of polarised light." It will be recollected, on the contrary, that during the total eclipse of 1868, it was observed both in Egypt and India that the light of the corona was strongly polarised. On future occasions this apparent discrepancy will undoubtedly receive such attention as it may be hoped will lead to a true solution.

Several photographs of the partial phase were obtained with beautiful definition. At Burlington, a record of the first contact was procured; and at Ottumwa, a picture at the last instant, just before totality, which gives a photographic record of the phenomenon known as Baily's beads, being, as Prof. Morton remarks, "simply the last glimpse of the Sun's edge cut by the peaks of lunar mountains into irregular spots." The pictures of the partial phase all show an increase of light on the solar surface in contiguity with the limb of the Moon, as was observed by De La Rue in 1860. Prof. Morton was at first inclined to attribute this to the existence of a lunar atmosphere; but subsequent experiments have led him to conclude that the cause is entirely chemical, and does not correspond to any celestial phenomenon; an analogous appearance is frequently to be seen in terrestrial photographs. It has been suggested by Mr. Stone, that there is a possibility of its being at least partially due to the scattering of the Sun's light which falls on the Moon's edge at a grazing angle, and that part of the light scattered is superadded to the direct light of the Sun near the Moon's limb.

Commander Ashe obtained four photographs of the totality at Jefferson Town, with an 8-inch equatoreal, the pictures being taken in the principal focus of the instrument. These pictures,

although they do not possess the sharpness of the American photographs, confirm what has already been stated in regard to the identity of form preserved by the protuberances and the entities with soft outlines; unfortunately in photographs 3 and 4, there is evidence of the disturbance of the telescope during the exposure of the sensitive plate.

Atmospheric Dispersion.

The Astronomer Royal has employed his great theoretical and practical knowledge of optics to correct the unpleasant, and for delicate observations, fatal effects of atmospheric chromatic dispersion at low altitudes. By providing prisms of different angles the mean effects of the chromatic dispersion can be entirely destroyed. In the *Monthly Notices* for January, an ingenious and simple method of applying a prism of varying angle has been described. It is certain that with eye-pieces of this construction, the difficulties in making delicate observations at low altitudes will be diminished.

Spectrum Analysis.

Our Fellow, Mr. Lockyer, has pursued with great diligence and success his spectroscopic researches on the Sun. Since the last Report he has found that several other substances besides hydrogen are occasionally to be detected above the photosphere, namely, sodium, barium, magnesium, and iron. The presence of the bright lines of these substances rising above the photosphere, and associated with the bright lines of hydrogen, Mr. Lockyer regards as indicating a state of disturbance of the solar matter, greater than was present at the time of his earlier observations. From a slight alteration in refrangibility of the bright lines of hydrogen, as shown by their want of perfect coincidence with the corresponding lines of absorption, Mr. Lockyer believes that he has evidence of rapid currents in the solar matter, sometimes attaining a velocity of 40 miles per second in a vertical direction, and a velocity of 120 miles per second in a horizontal or cyclonic direction. He considers that his observations support the conclusion that "the chromosphere and the photosphere form the true atmosphere of the Sun, and that, under ordinary circumstances, the absorption is continuous from the top of the chromosphere to the bottom of the photosphere." In these observations Mr. Lockyer is accumulating a store of facts, upon which we may hope to base a more complete theory of the constitution of the Sun than we now possess.

In the early part of last year Mr. Huggins, by the simple method of using a wide slit, afforded a means of observing directly the forms of the prominences.

Zöllner has proposed a new form of spectroscope, in which the

light of a star is divided between two prisms, each half of the light being refracted in an opposite direction. By this means small differences in position of stellar and solar lines appear doubled in amount, and can be observed, therefore, and measured with great accuracy. Zöllner, by using a wide slit, has made careful observations of the forms of the solar prominences, and of the rapid changes which take place in them. His observations confirm the conclusions as to the very great mobility of the matter of the red flames which had been previously arrived at by Janssen and Lockyer from the rapid changes they had observed in the bright lines of these objects.

Mr. Plummer observed successfully the spectrum of the Aurora Borealis in April 1869. His observations agree with the previous spectroscopic examination of this object by Angström and Struve.

Spectroscopic observations of the Aurora have also been made by Prof. Winlock.

Father Secchi has continued his spectroscopic researches on the Sun and stars. They are described in the *Comptes Rendus.*

Lunar Radiation.

The thermo-electric pile has been employed by more than one observer during the past year in experiments upon lunar radiation, and with some success. By means of the "three-foot" Parsonstown reflector, a thermopile, and a Thomson's reflecting galvanometer, a second thermopile being also connected with the galvanometer to compensate for disturbing effects, the Earl of Rosse succeeded, during the lunations of March and April 1869, in obtaining evidence of lunar heat increasing in amount with the phase of illumination. The manifested heat-rays were, however, shown to be principally obscure; and Lord Rosse hence infers that the greater part of the heat received from the Moon consists of solar heat that has been first absorbed by the lunar crust, and then given off in dark radiation. No evidence of cosmical heat was obtained. By comparison of the solar and lunar actions upon the pile, a vessel of hot water being used as an intermediate standard of reference, it was estimated that the relation of solar to full-Moon radiation is as 89819 to 1, a proportion sensibly the same as that arrived at by a purely theoretical investigation. By the use of a cistern of hot water, with a blackened exterior, exposed to the pile under similar circumstances to the Moon-lit surface of the great mirror, Lord Rosse found that the amount of deviation which the full Moon caused in his galvanometer appeared to indicate an elevation of temperature of the lunar surface through 500° Fahrenheit.

Some later observations have been made upon the same subject in Paris, respectively by M. Baille, at the Ecole Polytechnique, and M. Marié-Davy at the Paris Observatory. The

former employed a concave mirror of 39 centimetres aperture to condense the Moon's rays upon his pile, and also made use of a Thomson's galvanometer. The one conclusion at which he arrived was, that the full Moon, at Paris and in the summer months, gave as much heat to his pile as a radiating surface 6·5 centimetres square, maintained at boiling-water temperature and placed at a distance of 35 metres. M. Marié-Davy has published results of two series of measures secured during the lunations of October and November last. The first were made with a pile attached to a 9-inch equatoreal refractor, the second with an 8-inch mirror; the object-glass in the former case having been found to intercept a large proportion of the heat-rays. M. Marié-Davy's measures confirm those of Lord Rosse. They show that the heating effect of the Moon increases with the illumination of the visible disk. Between October 9, when the Moon was four days old, and October 20, when it was full, the measured heat of the condensed beam of moonlight increased from $0^\circ{\cdot}00017$ (Centigrade) to $0^\circ{\cdot}00287$. If this last number be divided by the ratio of the area of the concentrated image to the area of the object-glass, we have twelve-millionths of a centigrade degree as the direct heating power of the full Moon at the Earth's surface. This is the result given by the object-glass: that afforded by the mirror is about six times as great. It will be seen that M. Marié-Davy has converted his galvanometer indications into centigrade equivalents: how this conversion was effected, and how the constancy of the scale indications is secured, if it is secured, we are not informed. He confirms Lord Rosse's inference that the proportion of solar to lunar radiation is about as 80000 to 1, and likewise concludes that the Moon imparts to us no heat from an internal or cosmical source. Further, he infers that the diffusive power of the lunar surface is considerable, at least equal to that of the least coloured of terrestrial rocks; and he finds that the lunar heat by reason of its large percentage of obscure rays is far more impressionable by atmospheric humidity than that from the Sun.

It will be remembered that Professor Smyth, in his Teneriffe experiments, determined the heating power of the full Moon to be equal to one third of that of a Price's candle at a distance of 14 feet 9 inches. M. Marié-Davy finds that such a candle at such a distance affects his pile to the extent of $0^\circ{\cdot}00075$ centigrade, which he conceives to be the heating power of the Moon upon the summit of Teneriffe, upon the supposition that the heat emitted by the respective candle-flames was sensibly the same.

Heating Power of the Stars.

The experiments upon this subject, commenced by Mr. Stone with the great Equatoreal of the Greenwich Observatory, in the year 1868, and mentioned in the last Annual Report, were continued

Ephemeris of the Satellites of Uranus. By A. Marth, Esq.

Angles of Position at 8^h Greenwich Mean Time.

1870.	Ariel.	Umbriel.	Titania.	Oberon.
	°	°	°	°
Feb. 18	152	70	317	161
19	3	346	272	137
20	217	258	223	109
21	79	172	185	77
22	303	85	150	46
23	157	357	108	19
24	8	273	60	357
25	223	182	17	334
26	87	100	342	309
27	309	7	305	281
28	162	288	257	248
Mar. 1	13	193	211	[illegible]

as weather permitted during 1869. In pursuing them, Mr. Stone was led to the construction of a thermopile which there is good reason to believe will prove of value in thermometric researches other than those of the class for which it was specially prepared. His early trials convinced him that it was almost impossible to distinguish the feeble currents generated by stellar heat from the grosser effects produced upon the pile by exposure of one face within the telescope tube and the protection of the other face outside the telescope tube. It was evident that to maintain the pile in thermal equilibrium its two faces must be exposed to precisely similar atmospheric influences. Mr. Stone therefore resorted to what in effect may be described as a horseshoe pile, the two faces of which being similarly presented to the object-glass of the telescope were affected alike by disturbing causes, whether these took the form of draughts of air or cooling of the metals of the pile by radiation. By this arrangement the whole heat of a star's image cast upon either face of the pile manifested itself *per se.* Decided indications of heat from *Arcturus* and α *Lyræ* were thus obtained on several nights. The amounts were measured by a reflecting galvanometer; and by a somewhat tedious process the galvanometer indications were converted into Fahrenheit-scale equivalents. It was found that the heating effect of *Arcturus*, after allowing for absorption by the object-glass, was 0·00000137 of a Fahrenheit degree; that of α *Lyræ* being about two-thirds of this amount. Otherwise expressed, the heat from *Arcturus*, at an altitude of 25° at Greenwich, is about equal to that from a three-inch cube of boiling water at a distance of 400 yards, while from α *Lyræ* it is equal to that from the same cube at 600 yards. Mr. Stone conceives that the difference of heating power may be connected in cause with difference of colour. He finds that the manifested heat diminishes rapidly as the amount of moisture in the air increases, and that all sensible effect is cut off by the slightest cloud or haze. The details of the investigation are published in the *Proceedings of the Royal Society* for January, 1870.

Transit of Venus, 1874.

The preparatory arrangements for the observations of the transit of *Venus*, 1874, are sufficiently advanced to encourage us to expect most important additions to the data already collected for the determination of our fundamental astronomical unit of length. The British Government has already placed at the disposal of the Astronomer Royal sufficient funds for the equipment of five stations, each with an Altazimuth and Transit, for the determination of longitude and local time, and with two telescopes, one of which is to be of six inches aperture, and to be

provided with driving power. Four of the larger instruments have already been obtained, and the Transits and Altazimuths are in actual progress in the manufactory of Messrs. Troughton and Simms. The second telescope at each station will be of 4 inches aperture.

The present intention appears to be to place the British observers at Kerguelen's Island, Woahoo, Auckland in New Zealand, Alexandria, and Rodriguez.

A Commission, consisting of Admiral Paris, MM. Faye, Laugier, Villarceau, and Puiseux, has reported to the Bureau des Longitudes that it would be particularly desirable for the French astronomers to occupy the Islands of Saint Paul and Amsterdam, Yokohama, Tahiti, Nouméa, Mascate and Suez.

The North German astronomers have referred the consideration of the action which they should urge upon their Government, to a Committee, of whom the illustrious astronomer of Gotha was elected chairman.

This Committee appeared to lay great stress upon the employment of heliometers for fixing the relative position of *Venus* on the Sun at the different stations.

Equatoreals with driving power for the eye-observers were to be of 6 feet focal length and 52 lines aperture. The employment of photography and spectroscopic observations was discussed. The chief objection to the employment of photography appeared to be in the question of expense, although Professor Argelander was not satisfied with the degree of accuracy which might be expected from it. The further consideration of this question was, however, deferred to a sub-committee. Spectroscopic observations were only proposed to indicate the approach of the planet for the observation of external contacts. The Committee recommended that the Government be urged to fit out four expeditions,—two to the North and two to the South. The stations specially referred to as favourable were, Nertschensk, Hohodadi, Kerguelen, Edwards', Crozet's, and Auckland Islands, and in certain respects Mauritius.

The Russian territories offer most valuable positions for the location of observers. These stations are certain to be well occupied by the Russian astronomers. The Director of the Imperial Observatory at Pulkova has already secured a Committee for the consideration of a proposition to establish a chain of observers across the country from Kamtschatka to the Black Sea, at intervals of about 100 miles. This appears desirable on account of uncertainties connected with the atmospheric conditions in the month of December.

It is to be hoped that, for the same reason, there will not be a too great crowding of the observers towards one or two points, in other regions, to the exclusion of others of nearly equal importance; and that, in this matter at least, after the best consideration has been given to the subject, we may all adopt instruments of not very unequal power, and attempt to make the same class of

observations in the same way. Uniformity in these observations means success; want of uniformity, comparative failure.

We would here recall attention to the beautiful series of maps by Mr. Proctor of the parts of the Earth turned towards the Sun at the principal phases of the Transit, which accompany his paper in the March Number. This paper contains a careful deduction from Mr. Hind's results in the *Comptes Rendus*, vol. liii. p. 131, of the chief stations for the observations of internal contact, deduced with direct and especial reference to that phase as the most important point of observation. On account of the nature of the motion of the planet relatively to the Sun, this exhibits in a more striking light than had previously been done, the value of Halley's method in the Transit of 1874.

Re-observations of the Stars down to the 9th magnitude in Argelander's Zones from N.P.D. 10° to 92°.

Dr. Auwers has lately distributed a catalogue of 539 stars to be used in connexion with the proposed re-observation on the meridian of stars within the zones of N.P.D. 10° and 92° down to the 9th magnitude. This re-observation has been recommended by the German Astronomical Society. The scheme states that sufficient accûracy will be obtained by two observations of each star. Each observation to be taken over three or four wires, and the circle readings to be made with two microscopes. The work has been distributed amongst thirteen observatories. One zone, that from N.P.D. 35° to 40°, alone remains unappropriated.

34 *Messier.*

Dr. O. A. L. Pihl has lately republished with additions his paper on this cluster which originally appeared in the Supplementary *Monthly Notices*, for 1868. With an equatoreal of only 4¼ inches aperture, he has found an interesting and useful field of investigation by laying down the relative positions of 85 stars in this cluster. The positions are fundamentally referred to two principal stars as zero points, and very considerable accuracy appears to have been obtained. A re-examination of this cluster at some future time, and a comparison with Dr. Pihl's positions, may lead to important results.

The Newton-Pascal Forgeries.

"L'Examen de la Discussion soulevée au sein de l'Académie, par M. Leverrier," contains a complete exposure of the falsehood of the Newton-Pascal papers which M. Chasles was induced by the skill of an artful forger to communicate in such profusion

to the French Academy. M. Leverrier deserves great praise for the combination of talent, scientific skill, and zeal in the cause of truth which proved so efficient in finally destroying this imposture. However, the first indisputable astronomical proofs of the falsity of these documents are contained in the two letters which Prof. Grant of Glasgow addressed to M. Leverrier on September 12, 1867, and October 31, 1867, and which were published without delay in the *Comptes Rendus.*

Prof. Grant's objection of the identity of the so-called results of the masses, &c. of the planets with the corresponding results contained in the third edition of the *Principia*, was unanswerable.

We may thus assert that, in conjunction with that of his distinguished friend, Sir David Brewster, the name of our Fellow, Prof. Grant, will ever remain most honourably connected with the exposure of the Newton-Pascal imposture.

The Great Newall Telescope.

The 25-inch equatoreal telescope, commenced several years ago by T. Cooke & Sons, of York, for R. S. Newall, Esq., of Gateshead, is now so far completed that it has been removed from the works at York into its observatory in Mr. Newall's grounds, at Ferndene.

The general design and appearance is the same as that of Cooke's well-known equatoreals on the German principle improved; but the extraordinary size of all the parts has necessitated the special arrangement of most of them.

The length of the tube, which is of riveted steel plates, including dew-cap and eye-end, is 32 feet, and it is of cigar shape; the diameter of the object-end being 27 inches, and the centre of the tube 34 inches. The cast-iron pillar supporting the whole is 19 feet in height from the ground to the centre of the declination axis, when horizontal; and the base of it is 5 feet 9 inches in diameter. The trough for the polar axis weighs alone 24 cwt.: the weight of the whole instrument being nearly 9 tons.

The object-glass has an effective aperture of 25 inches (nearly). In order as much as possible to avoid flexure from the unequal pressure of the object-glass, it is made to rest upon three fixed points in its cell, and between each of these points are arranged three levers and counterpoises round a counter-cell, which act through the cell direct on to the glass, so that its weight in all positions is almost equally distributed among the 12 points of support, a slight excess being thrown upon the 3 fixed ones. The focal length of the object-glass is 29 feet. A Barlow lens is arranged to slide on a brass framework within the tube. The hand is passed through an opening in the side of the tube, and by means of a handle attached to the cell the lens may be pushed into or out of the cone of rays.

Attached to the eye-end of the tube are two finders, each

having an object-glass of 4 inches aperture; they are fixed above and below the eye-end of the main tube, so that one may be readily accessible in all positions of the instrument. It is also supplied with a telescope having an object-glass of 6½ inches aperture. This is fixed between the two finders, and is for the purpose of assisting in the observations of comets and other objects for which the large instrument is not so suitable. This assistant telescope is provided with a rough position circle and micrometer eye-pieces, and is illuminated by Cooke's new illuminating apparatus.

The driving clock is in the upper part of the pillar, and is of comparatively small proportions, the instrument being so nicely counterpoised that a very slight power is required to be exerted by the clock, through the tangent screw, on the driving wheel (7 feet diameter), in order to give the necessary equatoreal motion. The clock is regulated by a pendulum, the intermittent motion of which is converted into a continuous one by the peculiar and ingenious arrangement of a double train of wheels, invented and exhibited to the Society by the late Mr. T. Cooke; with the exception that a fan, revolving in an air-box, the sides of which are perforated with holes at regular intervals, alternately opened or closed by the action of the clock as it goes faster or slower, is substituted for the double spring which acted as a brake, a most elegant contrivance, first, it is believed, employed by the late M. Foucault.

The declination axis is of peculiar construction, necessitated by the weight of the tubes and their fittings, and corresponding counterpoises on the other end, tending to cause flexure of the axis. This difficulty is entirely overcome by making the axis hollow, and passing a strong iron lever through it, having its fulcrum immediately over the bearing of the axis near the main tube, and acting upon a strong iron plate rigidly fixed as near the centre of the tube as possible, clear of the cone of rays. This lever, taking nearly the whole weight of the tubes, &c., off the axis, frees it from all liability to bend.

The hour-circle on the bottom of the polar axis is 26 inches in diameter, and is divided on the edge,* and read roughly from the floor by means of a small diagonal telescope attached to the pillar; a rough motion in right ascension by hand is also arranged for by a system of cog-wheels moved by a grooved wheel and endless cord at the lower end of the polar axis, so as to enable the observer to set the instrument approximately in right ascension by the aid of the diagonal telescope.

The declination and hour-circles will be illuminated by Geisler's tubes, and the dark and bright field illuminations for the micrometers will be effected by the same means.

Mr. Norman Lockyer, who had an opportunity of examining

* The hour-circle is also divided on the face, and read by micrometer microscopes.

and trying this magnificent instrument shortly before its despatch to its owner Mr. Newall, speaks in the highest terms of its mounting and mechanical arrangements generally; and so far as he was able to judge of its optical performance, with the low power to which the state of the weather restricted him, he considered it to be very promising.

The difficulties attending the constructing and mounting of so large an object-glass are exceedingly great. That all these then have been entirely overcome no one conversant with the refinements involved can reasonably expect, and the optical and mechanical perfection of the famous Cooke factory has raised the standard by which telescopes are judged. We have every hope that this instrument will be worthy of their fame.

This record of enlightened private enterprise would be incomplete if we did not add that Mr. Newall proposes, when the instrument is properly fixed in a good climate, to place it at the disposal, for a certain number of hours in each day, of any qualified astronomer who desires to use it. The benefit likely to be rendered to science by Mr. Newall's liberality can hardly be overrated. We understand that his observatory is to be under the direction of Mr. Albert Marth. We have not yet heard that its site has been finally decided on; it is certain that the full powers of such a telescope cannot be called forth in the climate of England.

Mr. Buckingham's Telescope.

In connexion with this splendid accession to observational astronomy, we would call attention to the existence of a telescope not much inferior in size to that just described, which was constructed some years ago by Mr. J. Buckingham, a Fellow of this Society, and which was exhibited by him at the International Exhibition of 1862. Its clear aperture is 21¼ inches, and its focal length 28½ feet. It is mounted as an equatoreal of the (so called) German form. Mr. Buckingham has also constructed another telescope of 9 inches aperture, mounted equatoreally. Competent judges, who have been permitted to visit Mr. Buckingham's Observatory at Walworth, speak in the highest terms of the completeness and ingenuity of the arrangements. It is understood that Mr. Buckingham has employed a method, peculiar to himself, of practically correcting the aberrations of the large object-glass. Regarding its optical performance we have as yet no definite report. Mr. Buckingham is known to be not only a mechanician of great originality, but also an industrious observer. It is very much to be desired that he should find leisure from his arduous occupations to give to the world a com-

plete account of his optical processes, of the contrivances peculiar to his instrumental mountings, and of the performance and produce of the magnificent apparatus which he has created with so much labour.

Zenith Sectors for Indian Survey.

One of the zenith Sectors constructed for the Great Trigonometrical Survey of India, by Messrs. Troughton and Simms, from Colonel Strange's designs, has been despatched, and has reached its destination, Bangalore, in the Madras Presidency. It went out by the Overland route under charge of Lieut. Rogers, R.E., an officer of the Survey, and is said to have sustained no injury in transit. This instrument has a telescope of 4 inches aperture and 4 feet focal length. The sectors are portions of a circle 3 feet in diameter; they are read by four micrometer microscopes, illuminated by a simple light somewhat in the manner employed in the transit-circle of the Royal Observatory. It is furnished with the appliances of a zenith telescope, and can be used, therefore, as such if desired. The instrument is to be employed in the observation of latitudes on the southern portion of the great meridional arc of India at short intervals, with the twofold object of investigating the very interesting physical question of local attraction, and of eliminating the effects of such attraction on the geodetic determination of the figure of the Earth. These operations have been intrusted to Capt. J. Herschel, R.E.

Mr. Carrington's Observatory.

All who are interested in Astronomy will be glad to hear that our Fellow, Mr. Carrington, whose valuable observations of circumpolar stars and of Sun-spots are well known, is now engaged in erecting a new observatory at Churt, near Farnham. This observatory is arranged on a new plan of construction. As it is situated on a conical hill, entirely detached, and 60 feet high, no elevation of the building is needed. Mr. Carrington has sunk the observatory below ground, so that the instruments can just be directed over the soil at the top of the mound. There has been sunk a dry well, 6 feet in diameter, to a depth of 40 feet, in which a clock, placed in an air-tight case, may be kept at a position of invariable temperature and at a constant and diminished pressure. The observatory will be furnished with an Altazimuth on Steinheil's principle, in which the horizontal axis is also the optical axis; an object-glass of 6 inches, provided with a prism fixed outside it, is placed at one end, and the eyepiece at the other end of the axis.

We hope this observatory, when it is completed, will speedily be as well known for the valuable contributions to astronomy proceeding from it as Mr. Carrington's former observatory at Redhill.

Second Radcliffe Catalogue of Stars.

We have to record the publication by the Radcliffe Observer of a second catalogue consisting of 2386 stars, deduced from observations extending from 1854 to 1861. The observations were made with the transit instrument and the meridian circle, by Jones, the use of which has been discontinued since the establishment of the Carrington circle in 1861. This circumstance made it desirable to embody in a distinct catalogue the observations made with those instruments.

The stars contained in the Catalogue have been, in general, observed many times, and their places may therefore be considered to be fixed wiih the greatest accuracy. Mr. Main has instituted a most careful examination between the right ascensions of the principal stars and those given in the Greenwich Seven-year Catalogue for 1860. From this comparison corrections have been deduced and applied to the Radcliffe results, in order to reduce this Catalogue to the same fundamental epoch as that of the Greenwich Catalogue. A similar comparison has also been made between the resulting north polar distances of these catalogues, and a slight systematic discordance appears between the results which has been attributed, with great probability, to outstanding uncorrected errors of division in the circles.

The magnitudes assigned to the stars are in most cases deduced from observation, the number of estimates on which the final result being based is given in a separate column.

In the introduction it is stated that the whole of the work connected with the formation of the Catalogue has been made by Mr. Main and one assistant Mr. Luff.

Pulkova Observations.

The observations made with the Transit Instrument of the Pulkova Observatory, from 1842 to 1853, have been printed in two magnificent volumes, and distributed for the use of astronomers. The first volume contains a full and most interesting Introduction by the illustrious Director, Mr. Otto Struve, giving an account of the methods adopted and observations made for the determination and elimination of the errors of the instrument and of its position. In the same volume there is a Catalogue of the Mean Right Ascensions of 348 Stars reduced to the epoch 1845, with star-constants for two epochs, 1840 and 1870.

Chinese Astronomy.

The Assistant-Secretary, Mr. Williams, has presented to the Society a MS. work, entitled, *Comets observed in China from* B.C. 613 *to* A.D. 1640. These observations have been extracted from Chinese works of authority, and are given with the original text. They are translated and arranged chronologically, with a full explanation of the several asterisms, described as occurring in the path of the comets. They are preceded by some introductory remarks in which the progress of Chinese astronomy is traced from between two and three thousand years before the Christian era to the present time. There is also an Appendix, containing chronological and other tables, required in the reduction of Chinese time to our reckoning, the construction and use of which tables are fully explained in the Introductory remarks, with numerous examples of their application. To facilitate the identification of the asterisms mentioned in the observations, a Chinese celestial atlas has been constructed, traced from an original work on astronomy. In this the thirty-one divisions into which the Chinese divide the visible heavens are laid down separately with the stars composing them, according to our nomenclature.

Communications to the Society from February 1869 to February 1870.

1869.

Mar. 12. Observations of the Transit of *Mercury*, 1868. Sir T. Maclear.
- Ditto ditto Mr. Abbott.
- Ditto ditto Mr. Barneby.
- Occultations observed at Liverpool. Mr. Joynson.
- On the Solar Eclipse of August 7, 1869. Mr. Hind.
- On the Aden and Guntoor Photographs of Eclipse, 1868. Mr. De La Rue.
- Note on the Attraction of Ellipsoids. Mr. Cayley.
- On the Meteoric Shower, Nov. 1868. Sir T. Maclear.
- On the Transit of *Venus*, 1874. Mr. Proctor.
- Rotation of *Mars*. Mr. Proctor.
- On an extensive Train of Sun-spots. Mr. Browning.
- On an Improved Mode of Mounting Finders. Mr. Browning.
- On the Observations of the Transit of *Venus*, 1874. Mr. Airy.
- Personality in the Determination of the Line of Collimation in a Transit. Mr. Stone.

April 9. On the Determination of Longitude by the Electric Telegraph. Commander Ashe.

Note on an Aurora Borealis, April 2, 1869. Mr. Plummer.

On the Problem of the Determination of a Planet's Orbit from three Observations. Prof. Cayley.

Description of an Improved Driving-Clock. Mr. Kincaid.

On some Effect of the Comparative Clinging of the Limb of *Venus* to that of the Sun in the Transit of 1874, as compared with that of 1882. Mr. Stone.

On the Practical Speed of Electricity through 7200 Miles of Land Wire. Mr. Davidson.

Transit of *Mercury*, Nov. 1868. Mr. Nursingrow.

On Personality in Observing Transits of the Limb of the Moon. Mr. Dunkin.

May 14. Opposition of *Mars*.

On a Method of Imitating the Transit of an Inferior Planet. Mr. Hollis.

On Mr. Joynson's Paper of Occultations. Mr. Plummer.

On the Solar Eclipse of August 1869. Mr. Paine.

Determination of the Direction of the Meridian with a Russian Diagonal Transit Instrument. Capt. Clarke.

On a Sun-spot observed May 1, 1869. Mr. Bidder.

Probable Error of Greenwich Observations in Zenith Distances estimated by Discordances from the Separate Means. Mr. Stone.

Observations of Winnecke's Comet. Mr. Wortham.

On the Period of *η Argûs*. Prof. Loomis.

On ditto ditto Mr. Tebbutt, Jun.

On the Transit of *Venus*. Mr. Proctor.

On a Sun-spot, March 14, 1869. Mr. Browning.

On the Preparations desirable for Photographic Observations of Phenomena such as Transits of *Venus*. Col. Tennant.

Comments on the Preceding Paper. Mr. De La Rue.

On the Solar Eclipse of 1871. Col. Tennant.

Observations of Winnecke's Comet. Rev. S. Perry.

June 11. Theory of the Tides. Mr. Ogilvy.

Occultations of Stars by the Moon. Mr. Joynson.

Note on the Transit of *Venus*, 1874. Mr. Hind.

On the Distribution of the Nebulæ. Mr. Proctor.

On the Nature of certain Appearances in Sun-Spots. Mr. Brayley.

On a simple form of Star Spectroscope. Mr. Browning.

Note on Lambert's Theorem. Mr. Cayley.

Nov. 12. Note on the Floor of *Plato*. Mr. Birt.

Selenographical Notes, *Apennines*, and Adjacent Regions. Mr. Weston.

Lunar Eclipse of July 23, 24, 1869. Mr. Tebbutt.

On his New Observatory at Churt, Surrey. Mr. Carrington.

Occultations observed at Leyton. Mr. Talmage.
The November Star Shower, 1869. Mr. A. S. Herschel.
On some attempts to render the red prominences visible. Mr. De La Rue.
On the Solar Eclipse, 1869. Commander Ashe.
On a change in Colour in the Equatorial Belt of *Jupiter*. Mr. Browning.
On the Application of Photography to determining the Solar Parallax from Transits of *Venus*. Mr. Proctor.
On a Method of imitating the Transit of a Planet over the Sun. Rev. Dr. Robinson.
On the Solar Eclipse of 1869. Mr. Paine.
American Photographs, Total Solar Eclipse of 1869. Rev. T. W. Webb.

Dec. 10. Observations of the November Meteors, 1869. Lieut. Tupman.
Chinese Astronomy. Mr. Williams.
Determination of the Orbit of a Planet from Three Observations. Prof. Cayley.
On Annual appearances and their connexion with the Phenomena of Terrestrial Magnetism. Mr. B. Stewart.
A Method of constructing Charts by which in a few moments the great circle course between any two points may be accurately ascertained. Mr. Proctor.
A New Theory of the Milky Way. Mr. Proctor.

1870. Jan 14. Observations of Meteors, Nov. 13, 1869, in America. Mr. Davidson.
Observations of *Jupiter's* Satellites. Mr. Joynson.
On a Bright Cross Micrometer for measuring the position of lines in faint Spectra. Mr. Browning.
Occultation of Stars by the Moon. Captain Noble.
Summary of Sun-spot Observations made by the Kew Photo-Heliograph during 1869. Messrs. De La Rue, Stewart, and Loewy.
Seventh Catalogue of Double Stars observed at Slough, 1823–1829 inclusive, 84 of which had not been previously described. Sir John Herschel.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
The Lords Commissioners of the Admiralty.
Royal Society of London.
Royal Society, Edinburgh.
Royal Dublin Society.
Royal Asiatic Society.
Royal Asiatic Society, Bombay Branch.
Royal Society, New South Wales.
Royal Geographical Society.
Royal Institution.
Royal United Service Institution.
Royal Society, Tasmania.
Cambridge Philosophical Society.
Geological Society.
Photographic Society.
Society of Arts.
British Meteorological Society.
British Association.
Art-Union of London.
Institute of Actuaries.
British Horological Institute.
The Zoological Society.
Manchester Literary and Philosophical Society.
Liverpool Literary and Philosophical Society.
Literary and Philosophical Society, Leeds.
Philosophical Society, Glasgow.
The Free Library, Manchester.
Radcliffe Trustees.
Imperial Observatory, Paris.
Imperial Observatory, Vienna.
Imperial Observatory, St. Petersburg.
Royal Observatory, Munich.
Royal Observatory, Palermo.
Royal Observatory, Berlin.
Observatory at San Fernando.
Observatory, Coimbra.
Observatory, Berne.
Observatory, Prague.
Observatory, Collegio Romano.
Observatory, Sydney.
United States Naval Observatory.
Observatory, Cincinnati.
Observatory, Christiania.
L'Académie Impériale des Sciences de l'Institut de France.

Imperial Academy of Sciences, Vienna.
Imperial Academy of Sciences, St. Petersburg.
Royal Academy of Sciences, Berlin.
Royal Academy of Sciences, Göttingen.
Royal Academy of Sciences, Munich.
Royal Academy of Sciences, Amsterdam.
Royal Academy of Sciences, Brussels.
Royal Institute of Lombardy.
Royal Society, Copenhagen.
Royal Academy, Stockholm.
Imperial Society, Cherbourg.
Royal Institute, Palermo.
Academy of Sciences, Batavia.
Academy of Sciences, Bologna.
Instituto-Tecnico, Palermo.
Astronomische Gesellschaft, Leipsig.
American Philosophical Society.
American Association.
American Academy of Natural Sciences.
United States Naval Department.
Smithsonian Institution.
Franklin Institute.
Canadian Institute.
Editor of the Athenæum.
Editor of the Student.
Editor of the Quarterly Journal of Science.
Editors of Silliman's Journal.
Editor of Nation.
Editor of Cosmos.

C. Abbé, Esq.
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M. C. Lensser.
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M. Leverrier.
M. C. F. Lüther.
M. Von Littrow.
M. E. Mailly.
Prof. Mayer.
Rev. C. Mayne.
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M. H. Mohn.
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Dr. Oppolzer.

J. A. Parker, Esq.
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T. Warner, Esq.
Dr. E. Weisse.
Mr. J. Williams.
Dr. R. Wolf.
M. J. Zöllner.

ADDRESS

Delivered by the Chairman, Professor J. C. Adams, on Presenting the Gold Medal of the Society to M. Charles Delaunay.

Gentlemen,—It has been announced to you that the Society's Medal has been awarded to M. Ch. Delaunay for his great work on the Theory of the Moon.

The illness of our excellent President having made it impossible for him to be present on this occasion, the Council have done me the honour to request that I would occupy the chair, and in his stead lay before you the grounds of their award. I have acceded to their wishes with the more readiness because I have given some attention to special branches of the Lunar Theory, and my study of M. Delaunay's work has led me to form the highest opinion of its merits.

Of all the problems presented to us by physical astronomy none has so much engaged the attention of mathematicians as that of the determination of the motion of our satellite. The theoretical interest as well as the great practical importance of the results, has proved an irresistible attraction, and the mathematical difficulties have merely acted as a stimulus to the invention of various methods of surmounting them. It is fortunate that this has been the case, as the excessive labour involved in any theory of the Moon approaching to completeness, might otherwise have proved too great for human perseverance. The foundations of the theory were laid by Newton in his *Principia;* and although his investigations are only fragmentary, being simply intended to show

how some of the leading lunar inequalities may be deduced from theory, yet they form one of the most admirable portions of that immortal work. Towards the middle of the eighteenth century the theory was more systematically entered upon by Clairaut, D'Alembert, and Euler, who severally showed that the theory was competent to give very approximate values of all the inequalities which were then recognised by observation.

Still the theory was far from being sufficiently perfect to serve as a foundation for lunar tables accurate enough for the uses of navigation. This degree of accuracy was first attained by the tables of Mayer, who not only carried the approximations to the values of the coefficients of the various lunar inequalities further than his predecessors had done, but also corrected the theoretical coefficients thus obtained by comparison with his own observations. The theory was greatly advanced by Laplace, not only by his more accurate theoretical determination of the coefficients, but also by several important discoveries, especially that of the cause of the Moon's secular acceleration.

The improvements in the lunar tables, however, which were made successively by Bürg and Burckhardt, were founded, not on theory, but on comparison of the former tables with observations; and the empirical tables thus produced were far more accurate than any that could have been formed at that time by theory alone. Dissatisfied with this state of things, and wishing to see astronomy founded exclusively on the law of attraction, only borrowing from observation the necessary data, Laplace induced the Academy of Sciences to propose for the subject of the mathematical prize which it was to award in 1820 the formation, by theory alone, of lunar tables as exact as those which had been constructed by theory and observation combined. The prize was divided between two memoirs — one by M. Damoiseau, the other being the joint production of MM. Plana and Carlini. Damoiseau's memoir is printed in the third volume of the *Recueil des Savants Etrangers*. Plana's great work on the lunar theory, which appeared in 1832, is the development of the joint memoir by himself and Carlini. By these important works an immense advance was made in the theory, the approximations being carried to such an extent that the resulting coefficients were comparable in accuracy with those given by observation. In 1824 Damoiseau published tables founded entirely on his theory, which were found to be quite as exact as those of Burckhardt.

Both Damoiseau and Plana, following the example of Laplace, start from differential equations in which the Moon's longitude is taken as the independent variable; and after the equations have been integrated, they obtain the values of the Moon's coordinates in terms of the time by reversion of series. An important innovation, however, was introduced by Plana in the mode of conducting the investigation and exhibiting the results. The values of the Moon's co-ordinates being developed in series of sines and cosines of angles which vary uniformly with

the time, the coefficients of the several terms of these series will depend on the eccentricities of the orbits of the Sun and Moon, the inclination of the Moon's orbit to the plane of the ecliptic, the ratio of the mean motions of the Sun and Moon, and the ratio of their mean distances from the Earth. Now Damoiseau, in common with all previous writers, having assumed certain values of the quantities just mentioned as given by observation, contented himself with determining the numerical values of the coefficients. Although this is all that is required for the construction of tables, yet, from a theoretical point of view, it leaves the mind unsatisfied, inasmuch as any coefficient in its numerical form shows no trace of its composition, that is of the manner in which its value depends on the value of the assumed elements. The several coefficients are far too complicated functions of the elements to be represented analytically, except in the form of infinite series, and Plana, accordingly, developes these coefficients in such series, proceeding by powers and products of the eccentricities, the tangent of the inclination, the ratio of the Sun's mean motion to that of the Moon, and the ratio of the Moon's mean distance to that of the Sun, all these quantities being assumed to be small, and the last mentioned ratio, which is much smaller than the others, being considered as a quantity of the second order.

In this mode of development, the numerical factor which enters into any term of the coefficient of any of the lunar inequalities is an ordinary fraction which admits of being determined not merely approximately, but with absolute accuracy. It is easy to see what great facilities are afforded by this circumstance for the verification of the work by a comparison of the results obtained by different methods. The greater or less degree of approximation will thus depend on the greater or less number of terms taken into account in the several series.

The numerical values of the several elements are not substituted in the formulæ until the work is completed, and this is attended with the important advantage that when a comparison of the theory with observation has supplied more accurate values of the elements, their corrected values can be at once substituted in the same formulæ, without requiring any additional work.

On the other hand, if the numerical values of the elements be introduced into the calculations from the first, then if it is desired to introduce corrected values of the elements, much additional investigation will be required for the purpose.

No doubt the labour required in order to obtain a given amount of numerical accuracy by this method is very much greater than is required when each coefficient, instead of consisting of a series of terms, is reduced to a simple numerical quantity, but the great theoretical advantage of knowing the composition of every coefficient in terms of the elements well repays the additional labour.

The degree of convergence of the series obtained for the

several coefficients is in general sufficiently rapid, but in some few of the coefficients, on the contrary, the convergence is so slow, at least in the leading terms, that it is necessary to take into account terms which are analytically of a higher order than those to which the approximation is in general limited.

Thus Plana, who proposed to himself to determine the lunar inequalities completely to the fifth order, found it necessary in special cases to carry the approximation to the seventh and even to the eighth order, and in several cases he also added an estimated value of the remainder of the series founded on the observed law of diminution of the calculated terms.

Soon after the publication of Plana's great work, Sir John Lubbock formed the plan, which he partly carried out in his various tracts on the theory of the Moon, of verifying Plana's results by a totally different method, starting from differential equations in which the time is taken as the independent variable, and thus avoiding the necessity of reversion of series.

Later, M. de Pontécoulant undertook the same work on a similar plan, and carried it out more completely in the fourth volume of his *Théorie Analytique de Système du Monde.*

These works, while they corrected some errors which had crept into Plana's computations, confirmed their wonderful general accuracy, and with some few exceptions they do not extend the approximation beyond the order to which Plana restricts himself.

Meantime, M. Hansen had undertaken a completely new investigation of the lunar theory, by a remarkable method peculiar to himself and explained in his *Fundamenta nova investigationis orbitæ veræ quam Luna perlustrat*, which appeared in 1838.

In applying the method described in this work to the case of the Moon, M. Hansen throughout employs numerical values of the elements of the Moon's orbit, and consequently the coefficients of the lunar inequalities as obtained by him are also purely numerical. The process is one of successive approximations, which are repeated again and again until the values of the inequalities which are found from the last approximation sensibly coincide with those which were assumed in entering upon that approximation.

The numerical values of the coefficients thus finally obtained are undoubtedly very exact. The slight corrections which these coefficients still require are probably chiefly due to the small corrections required by the numerical elements on which the calculations are based, and in the method employed no provision is made for taking into account the effect of these corrections.

From his formulæ, M. Hansen constructed tables of the Moon, which were published in 1857, at the expense of the British Government; and these tables, having been found far superior in accuracy to all others, are now exclusively employed in the calculation of ephemerides.

A detailed account of the calculations leading to M. Hansen's

last approximation, was given by him in the two parts of his *Darlegung der Theoretischen Berechnung der in den Mondtafeln angewandten Störungen*, which severally appeared in 1862 and 1864.

After the great works, to which we have thus briefly referred, had been either completed or were in progress, it might have been supposed that the matter was exhausted.

Our Associate M. Delaunay, however, was not of this opinion. Having devised, so long ago as 1846, a perfectly original and singularly beautiful method of integrating the differential equations of the Moon's motion, he determined to apply this method to the complete re-investigation of the theory, and to carry on the approximation to a much greater extent than had been done by his predecessors. The principal fruits of his labours, to which he has devoted himself with almost unexampled perseverance for so many years, are contained in the magnificent volumes which the Imperial Academy of Sciences have done both M. Delaunay and themselves the honour of publishing among the volumes of their *Memoirs*. It is for this great work that your Council have awarded to M. Delaunay the Society's medal.

Strongly impressed with the advantages of determining the co-efficients of the lunar inequalities in the analytical form, both as affording a solution more complete in itself and more satisfactory to the mind, as well as one offering facilities for the comparison of the results of different investigations, M. Delaunay did not hesitate to follow the example set in this respect by M. Plana, notwithstanding the immense length of the necessary calculations. M. Delaunay's results are thus obtained in a form which makes them directly comparable with those of M. Plana, while the methods employed in obtaining them are wholly different.

M. Delaunay chooses the *time* as the independent variable, and takes as his starting-point the differential equations furnished by the theory of the variation of the arbitrary constants. In an able Memoir which appeared in 1833, Poisson had advocated the employment of these equations in the theory of the Moon's motion, and he applied them to the discussion of some special points of that theory. These equations had been long used, almost exclusively, for the determination of the perturbations of the planets, and they offer peculiar advantages in the treatment of the secular inequalities and those of long period. In the case of the Moon, however, in consequence of the large perturbations caused by the disturbing force of the Sun, the ordinary mode of integrating these equations by successive approximations soon leads into calculations of inextricable complexity. In fact, these equations give the differential coefficients of the several elliptic elements taken with respect to the time, in terms of the elements themselves. In the case of the planets, where the disturbing forces are so small compared with the predominant central force of the Sun, very approximate values of the disturbed elements may be found by substituting in the values of the differential coefficients,

the undisturbed instead of the disturbed values of the elements, and then integrating.

The perturbations of the elements thus found are said to be due to the first power of the disturbing force. If now the approximate values of the disturbed elements be substituted in the differential equations, and these be again integrated, we shall obtain a second approximation to the values of the disturbed elements, and the additional terms thus found are said to depend on the square of the disturbing force. In the theories of the planets it is only in special cases that terms depending on the square of the disturbing force need be taken into account, and it is scarcely ever necessary to consider terms of the next order of approximation.

In the case of the Moon, however, it would be necessary to repeat the process of approximation at least four or five times, in order to obtain results of the accuracy required in the present state of the theory. If we consider that the disturbing function consists of a great number of terms, and that each term gives rise to a corresponding term in the value of each of the disturbed elements, while powers and products of the corrections of all the elements in every possible combination, up to a certain order, have to be taken into account, it may be readily imagined how impracticable it would be by such a process to carry on the approximation to a greater extent than has been already done by Plana. Every process in which the approximations require to be repeated several times, is subject to the inconveniences that have been described, and these inconveniences are much greater when, as in the present case, we have to make successive approximations to the values of the *six* elements of the orbit, instead of to the values of the *three* co-ordinates of the Moon.

It was with the view of avoiding this excessive complication of the method of successive approximations that M. Delaunay devised his method of integrating the differential equations of the Moon's motion. The fundamental idea of this method consists in attacking the difficulty by small portions at a time, and in replacing these extremely complicated successive approximations by a much greater number of distinct operations, each of which is comparatively simple, so that it may be carried out to any degree of exactness that may be desirable, while the mind is relieved by being able readily to embrace the whole of each operation in one view.

It is difficult, without the use of algebraical symbols to give an idea of M. Delaunay's beautiful method, but I must endeavour, in some measure, to fulfil this task, and I must crave your indulgence should I fail in the attempt.

The theory of the variation of the arbitrary constants gives, as is well known, the differential co-efficients of the elliptic elements with respect to the time, in terms of the elements themselves and the partial differential co-efficients of a certain function, called the Disturbing Function, taken with respect to those ele-

ments. By a proper choice of elements, the differential equations may be reduced to their simplest, or to what is called their *canonical* form. In this form the six elements are divided into three pairs, the elements of each pair being conjugate to each other. Then the differential coefficient of any element with respect to the time is simply equal to the partial differential coefficient of the disturbing function taken with respect to the element which is conjugate to the former, the partial differential coefficients which occur in the two equations corresponding to a pair of conjugate elements being affected with opposite signs.

The disturbing function may be readily developed in a series of periodic terms involving cosines of angles, each of which is formed by the combination of multiples of the Moon's mean longitude, the distance of the Moon's perigee from its node, and the longitude of the node, together with angles which depend on the position of the disturbing bodies. The disturbing function likewise contains a non-periodic term, which, as well as the coefficients of the periodic terms, are all functions of the major semi-axis, the eccentricity and the inclination of the Moon's orbit.

Since the mean longitude of the Moon involves the time multiplied by the mean motion which is a function of one of the elements, it is obvious that the differentiation with respect to this element will give rise to terms in which the time occurs without its being included under a sine or a cosine. Such terms would render the equations very inconvenient for the determination of the lunar inequalities; and M. Delaunay accordingly avoids the introduction of them by taking the mean longitude itself instead of the *epoch* of mean longitude, as one of his elements, while by the simple yet novel expedient of adding to the disturbing function a non-periodic term which is a function of the major semi-axis alone and is independent of the disturbing forces, he preserves to the differential equations the same very simple form which they had at first. After this modification of the disturbing function, the time no longer enters into it explicitly except in so far as it is introduced by the values of the co-ordinates of the disturbing bodies, and consequently the difficulty which was before met with completely disappears.

The six elements employed by M. Delaunay are thus,—the Moon's mean longitude, the distance of the perigee of its orbit from the node, and the longitude of the node, which for distinction may be called the three *angular* elements, and three other elements which are respectively conjugate to the former, and which are determinate functions of the major semi-axis, the eccentricity and the inclination of the orbit.

The three co-ordinates of the Moon at any time are given in terms of the three angular elements and of the quantities last mentioned.

Now let us imagine, for a moment, that the disturbing function contained no periodic terms, but was reduced simply to its non-periodic part. Consequently the partial differential co-

efficients taken with respect to the angular elements would all vanish, and therefore the three conjugate elements would be all constant, as well as the major semi-axis, the eccentricity and inclination, of which those elements are functions. Hence, again, the partial differential co-efficients taken with respect to the conjugate elements would be functions of those elements, and would therefore be constant. Hence each of the angular elements would consist of an arbitrary constant and a term proportional to the time, the multiplier of the time in each case being a known function of the three constant elements.

The object of M. Delaunay's method is, by means of a series of changes of the variables, to cause all the more important periodic terms to disappear from the disturbing function, one by one, while the differential equations continue to retain their canonical form, so that after each transformation we approach more nearly to the conditions of the ideal case which has just been considered.

In order to effect any one of these transformations, M. Delaunay supposes, for the moment, that the disturbing function is reduced to its non-periodic part, together with one of the periodic terms selected from among those which have the greatest influence in producing the lunar inequalities. With this simplified form of the disturbing function, the equations admit of being easily integrated. The elements with which we start may thus be expressed in terms of three new angular elements which vary uniformly with the time, and three new constant elements. M. Delaunay shows how the constant elements may be so chosen that they may be considered as respectively conjugate to the three new angular elements, so that, in fact, the quantities which are multiplied by the time in the expressions of these angular elements are respectively equal to the partial differential co-efficients of a function of the new constant elements taken with respect to these elements.

Having thus found the relations between the old set of elements and the new ones by means of the simplified form of the disturbing function, M. Delaunay now restores the complete value of that function, and chooses new elements which are connected with the old ones by exactly the same relations as in the case just considered. Of course the three new angular elements will no longer vary uniformly with the time, and the three elements respectively conjugate to these will no longer be constant.

When, by means of the proper formulæ of transformation, the new variables have been substituted for the old ones in the disturbing function and in the expressions of the Moon's co-ordinates, M. Delaunay shows that—

1st. One of the important terms of the disturbing function disappears, viz., the periodic term which was selected in the preliminary investigation.

2nd. Various inequalities corresponding to this term are introduced into the values of the three co-ordinates of the Moon.

3rd. The values of the six new variables in terms of the time

are determined by differential equations of exactly the same form as those which determined the values of the six variables for which they have been substituted.

One of the periodic terms having been in this manner caused to disappear from the disturbing function, a new operation of exactly the same kind causes another term of this function to disappear; similarly a third term may be taken away by means of a third operation, and so on to any number of terms.

In this way, after a suitable number of operations of this kind have been effected, the disturbing function will have been simplified by the removal from it of its most important periodic terms, after which the further process of integration becomes simple enough to be treated in the same manner as if we were concerned with the perturbations of a planet or of the Sun.

The whole difficulty in the determination of the lunar inequalities is caused by the great magnitude of the disturbing force of the Sun. M. Delaunay has therefore at first confined his attention to the investigation of the irregularities which are produced by this disturbing force, and the two magnificent volumes before us are entirely occupied with this investigation. Thus he has provisionally left out of consideration the very small inequalities due to some secondary causes, such as the attraction of the planets and the figure of the Earth; and, besides, he has omitted to consider the perturbations of the Sun's apparent motion about the Earth, intending in a supplêmentary volume to take into account the effects due to these several causes.

By means of repeated applications of the beautiful method of transformation which I have above attempted to describe, M. Delaunay proceeds to get rid of all the periodic terms of the disturbing function due to the Sun's disturbing force, which are capable of producing inequalities in the co-ordinates of the Moon of an order inferior to the fourth. For this purpose fifty-seven such operations are required to be performed. When these have been effected, the periodic terms which remain in the disturbing function are so small that their powers and products may be neglected, and consequently the differential equations which determine the six elements last introduced in terms of the time, may be integrated at once. Since the values of the Moon's co-ordinates are known in terms of the elements just mentioned and the time, we have only to substitute the values of the elements that have been found, in order to determine the Moon's co-ordinates in terms of the time.

The values of the elements, however, that would be found in this way are very complicated, and therefore the substitutions which would be required in order to find the Moon's co-ordinates would be excessively long. M. Delaunay, accordingly, prefers to get rid of the remaining periodic terms in the disturbing function, one by one, by means of transformations exactly similar to those which have been already effected. In order to carry on the approximation to the extent which he desires, M. Delaunay finds *it necessary* to perform no less than 448 of these secondary

operations, but each such operation becomes very simple, since the squares of the coefficients of the periodic terms under consideration may be neglected.

Thus, at length, by means of 505 transformations, all the periodic terms of the disturbing function are removed, and the problem is reduced to the ideal case which was considered at the outset of our account of M. Delaunay's method.

After each transformation, by making the proper substitutions in the expressions for the Moon's co-ordinates, those co-ordinates are obtained in terms of the system of elements last introduced, so that finally the three co-ordinates are known in terms of the three final constants and angles which vary uniformly with the time.

It has been already mentioned that Plana, in his great work on the Lunar Theory, determined the analytical values of the coefficients of the lunar inequalities as far as terms of the fifth order inclusive, and that he only carried on the development to a greater extent in cases where the slowness of the convergence of the series appeared to him to render it necessary to take into account terms of higher orders than the fifth.

M. Delaunay has proposed to himself to carry on the approximation so as to include all terms of the seventh order, and in cases where the series converge slowly to take into account terms of the eighth, and even of the ninth order.

Those who have had any experience in calculations of this nature will readily understand how enormously the labour required has been increased by thus adding two orders more to those which Plana has considered. It is not merely that the terms of higher orders are far more numerous than those of the lower, but also that each of the terms of the former kind is much more difficult to calculate, since it arises from a much greater number of combinations of terms of the inferior orders.

This enormous labour, which has occupied M. Delaunay for nearly twenty years, has been performed by him without assistance from any one. Indeed, from the nature of the calculations which are required, it would not have been easy to obtain any effective assistance. In order to insure accuracy, M. Delaunay has omitted no means of verification, and he has performed all the calculations, without exception, at two separate times, with a sufficient interval between them to prevent any special risk of committing the same error twice in succession.

The volumes before us are perfect models of orderly arrangement. Notwithstanding the great length and complication of the calculations, the whole work is so disposed that any part of it may be specially examined with the utmost readiness by any one who may wish to test its accuracy.

Finally, the analytical expressions which have been obtained for the Moon's co-ordinates are converted into numbers, by substituting for the elements the most accurate numerical values which the comparison of theory with observation has made known.

Such is an imperfect sketch of M. Delaunay's labours on the

Theory of the Moon contained in these two magnificent volumes, the former of which appeared in 1860, and the latter in 1867. As I have already stated, they do not include a complete theory of the Moon, but only that which is by far the most difficult and complicated part of that theory, viz., the investigation of the perturbations due to the direct action of the Sun supposing its apparent motion about the Earth to be purely elliptic. Of the investigations which are required to take into account the remaining very small causes of disturbance, and which are intended by M. Delaunay to be included in a supplementary volume, some of the most important have been already completed by him, particularly the calculation of the Secular Variation of the Moon's Mean Motion, and the investigation of the long inequalities due to the action of *Venus*.

I understand also that M. Delaunay is engaged in the construction of new Lunar Tables founded upon his theory.

Your Council, however, has decided that we ought not to await the appearance of M. Delaunay's supplementary researches before we mark emphatically our sense of the value of his labours.

The present work is complete in itself; in it the very difficult and complicated problem of determining the Moon's motion is attacked by a perfectly original method, and that one as powerful and beautiful as it is new. The work has been planned with admirable skill and has been carried out with matchless perseverance. The result is an enduring scientific monument of which our age may well be proud, and which we are happy to distinguish, on this occasion of our fiftieth anniversary, with the highest marks of our approval which it is in our power to bestow.

The Chairman, then delivering the Medal to M. Delaunay, addressed him in the following terms:—

M. Delaunay, il ne me reste plus maintenant qu'à vous présenter cette médaille au nom de la Société Royale Astronomique, qui désire par ce tribut vous exprimer la haute appréciation qu'elle a de vos travaux. Notre Président regrette vivement que l'état de sa santé l'empêche de remplir cette tâche agréable. Il m'a prié de le remplacer dans cette circonstance, et je le fais avec d'autant plus de plaisir que depuis bien long-temps j'ai la plus grande estime pour vos hauts talents, et que j'ai étudié vos belles recherches avec la plus grande admiration, aussi je suis heureux de vous exprimer que notre Société vous a suivi dans votre immense travail avec le plus vif intérêt; et quoique ce travail ne soit pas entièrement terminé, elle sent qu'elle ne peut tarder plus long temps à reconnaître la haute valeur de vos recherches. Nous sommes heureux de vous voir au milieu de nous à cette occasion, et nous faisons des vœux pour que votre santé et vos forces puissent durer de longues années afin d'enrichir la science de plus en plus du fruit de vos grands talents.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

WILLIAM LASSELL, Esq. F.R.S.

Vice-Presidents:

G. B. AIRY, M.A. F.R.S., Astronomer Royal.
WARREN DE LA RUE, Ph. D. F.R.S.
Rev. ROBERT MAIN, M.A. F.R.S., Radcliffe Observer.
Admiral R. H. MANNERS.

Treasurer:

SAMUEL CHARLES WHITBREAD, Esq., F.R.S.

Secretaries:

WILLIAM HUGGINS, Esq. F.R.S.
EDWARD J. STONE, Esq. M.A. F.R.S.

Foreign Secretary:

Lieut.-Col. ALEXANDER STRANGE, F.R.S

Council:

J. C. ADAMS, M.A. F.R.S., Lowndean Professor of Astronomy, Cambridge.
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EDWIN DUNKIN, Esq.
GEORGE KNOTT, Esq.
J. NORMAN LOCKYER, Esq. F.R.S.
Captain WILLIAM NOBLE.
Rev. CHARLES PRITCHARD, M.A. F.R.S., Savilian Professor of Astronomy, Oxford.
BALFOUR STEWART, Esq. M.A. LL.D. F.R.S.

Dome for Sale.

Captain Swann, the present proprietor of Redhill Observatory, wishes to part with the dome, which was put up by Mr. Carrington at an expense of more than 150*l.* He will take 50*l.* for it; it is in as good condition as when first erected. It is of iron throughout, 25 feet diameter.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *March* 11, 1870. No. 5.

WILLIAM LASSELL, Esq., President, in the Chair.

Charles Meldrum, Esq., Government Observatory, Mauritius; and
Rev. H. C. Watson, Trinity College, Cambridge,

were balloted for and duly elected Fellows of the Society.

Dark Objects Crossing the Sun's Disk.

An unusual phenomenon was noticed by Lieut. Herschel while observing the Sun at Bangalore, in India, on the 17th and 18th of October last, the following particulars of which are extracted from a letter to his brother, Prof. A. S. Herschel, dated Bangalore, October 20–25th, 1869. At about noon on the 17th, while preparing to observe the red prominences of the Sun, with an equatoreal refractor of 5 inches aperture armed with a spectroscope, Lieut. Herschel first threw the Sun's image on a sheet of white cardboard, placed as a screen, to obtain a general view of any spots which might be visible on its disk. Some dark shadows were soon noticed crossing the Sun, and afterwards some light streaks beyond its border. The first were attributed to birds, and the second to sparks inside the tube; but their frequency first, and then their uniformity of direction attracted consideration, as evidently indicating that an unusual phenomenon was in progress, and a few minutes' attention showed that what were dark shadows on the Sun were luminous moving images beyond its border. The possibility of the passage of a meteoric stream having here suggested itself, the Sun's image was sketched, and a pencil was drawn across it wherever a shadow passed. In ten minutes

thirty or more lines were drawn, and their accordant direction proved that it was really a continuous stream.

The clock-work was now adjusted, and a friend's assistance was obtained to move the screen, so as to keep the Sun's image fixed upon it by means of the positions of images of conspicuous Sun spots; while, whenever a shadow appeared a ruler was placed, and a line was instantly drawn in the direction which it had taken. After about ten minutes the observers changed places, and thus secured two diagrams containing forty-four and thirty-eight lines respectively, and noted the N. and S. direction by shifting the image of a solar spot in declination. The apparent sizes of the shadows were defined by three marks upon the margins of the diagrams. A similar image of the Sun being next cast with the large finder of the equatoreal, it was found that the shadows crossed that as well. On diligently looking through the finder, and also through the main tube of the telescope, the images were at length visible there, not as shadows crossing the Sun's disk, but as ill-defined passing sparks near the Sun's border. They were thus seen and repeatedly examined by both of the observers until the Sun set.

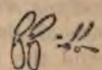
At seven o'clock on the following morning the appearances were the same, the bodies still passing in a continuous stream. Fresh drawings were made; and it was found possible to obtain views of them in the spectroscope. Soon after noon on the 18th, the following principal features, or apparent characters of the bodies, were recorded. "1. Their direction is towards about 150° E. of North, but it is almost certain that there are two streams. 2. They are not very distant. The majority of them are completely out of focus when the Sun is in focus. When focus was adjusted on a passing cloud they all appeared much better defined. 3. They are brightest near the Sun, as well as most frequent; in spite of the overpowering tendency of the Sun's light they distinctly lose their brightness as they leave the Sun, and acquire it as they approach. 4. They vary greatly in size and velocity, and in distinctness of definition. As a rule the smaller they are the slower they move, and the more distinct is their form; but there are exceptions. The slower ones can be followed up, by casting loose, and may be traced several degrees from the Sun. 5. Their motion is exceeding irregular; not to the extent, however, of *in the least degree* making their average direction and velocity uncertain, but only in comparison with that regularity which is to be expected in cosmical matter. One only out of hundreds (if it was one) was retrograde in direction. Not unfrequently, however, their path is contorted or devious. On one occasion (the most marked instance that I can recollect) one entered the field slowly in the usual direction, but on reaching the centre seemed to meet a transverse current, with which it was swept away at right angles to its former course. On the whole the motion resembled that of floating particles, subject to the influence of a mingling of many currents. 6. Their number is

anything short of infinity. Fifteen or twenty in a minute were repeatedly counted across the field of view, 45′ in diameter. 7. Their form is very difficult to describe. For a long time the impression was as of a half-moon moving diameter forwards, or sometimes edgeways. Then the feeling was that there were large luminous snowflakes of various sizes, the smaller ones being almost stellar in their distinctness and brilliancy. But since I tried focussing on a distant cloud they seemed with one accord to take a tangible and real shape, a kind of double crescent with a bar across, and wings or phantom-like appendages accompanying, thus—

Whatever was their shape, it had reference to the direction of their motion, and not to the Sun. 8. Their spectrum is solar. Vivid flashes from top to bottom are seen as they cross the slit."

Later in the afternoon, having by continued effort managed to see them without reflecting eye-piece or dark-glass almost up to the very edge of the Sun, with a power of 55, Lieut. Herschel almost satisfied himself that that shape which he had seen so distinctly was a delusion, and that the real shape was a disk; when light clouds, passing over, at first interrupted a perfect view of the objects, but eventually solved the question of their nature. The particles streaming by in regular direction were intensely brilliant, and many of them moved so slowly as to take many seconds crossing the field of view.

At last one of the objects *paused*, *hovered*, and whisked off, and in that instant the observer writes that he saw—



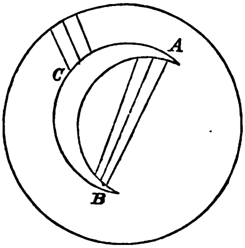
"There was no longer any doubt; they were locusts, or flies of some kind. The next morning (October 19th) they were still streaming by in hundreds in the same direction; but I paid little attention to them now, but put up the spectroscope to look at prominences. After a while they were passing the slit so frequently that I might have counted twenty or thirty in a minute. It remains to be seen if the appearances will continue. As it was, the continuous flight for two whole days, in such numbers, in the upper regions of the air, of beasts that left no stragglers is a wonder of natural history, if not of astronomy."

At the time when the above description was written, the *Homeward Mail* contained the news that countless locusts had descended upon certain parts of India. An appeal has also recently been made in the daily newspapers, stating that a famine has arisen at Jerusalem on account of the destruction of every green herb there by the devastations of innumerable locusts.

Among the appearances which may, perhaps, receive a partial explanation from the above observations of Lieut. Herschel, are some very similar phenomena recorded by the observers of the total solar eclipse on the 7–8th of August last at some of the stations in the United States of America.* At Ottumwa, "about

* *Journal of the Franklin Institute*, for 1869, p. 200, *et seq.*

twenty-five minutes before totality, Prof. Zentmayer observed some bright objects on the ground glass, crossing from one cusp to the other of the solar crescent, as indicated by the accompanying



cut by the lines from A to B; each object occupied about two seconds in passing, and they all moved in right lines, nearly parallel, and in the same direction. These points were well defined, and whatever they might be, must (in order to produce such sharply defined images on the ground glass) have been several miles distant from the telescope. After calling Prof. Himes' attention to this phenomenon, and observing some eight or ten bodies in all, Mr. Zentmayer then noticed three others coming in from the limit of the field, and disappearing in the solar crescent, as shown at C; but not reappearing on the other side. It is worthy of note that the direction of motion of the three bodies last mentioned coincided with that of the wind blowing at the time, but that of the others did not; and they are thus, as also for other reasons, unlike the plant seed noticed some years ago by Mr. Dawes."* Prof. Coffin also saw "meteoric bodies cross the telescope from east to west like bright flakes." †

The eclipse having taken place very near the periodical date of the 10th of August, it is interesting to remark that at Vevay, Indiana, during the eclipse, several meteors were seen, at an altitude of about 45°, taking a westerly direction. On the other hand the bright objects seen near the Sun's disk by Prof. Zentmayer, and the bright flakes noticed by Prof. Coffin, were not impossibly caused by the distant passage between the observer and the Sun's disk of some winged tribe as the extraordinary flight of locusts, seen through the telescope by Lieut. Herschel, in India, in October last.

Notes on the Solar Corona and the Zodiacal Light; with suggestions respecting Observations to be made on the Total Solar Eclipse of December 24th, 1870. By Richard A. Proctor, B.A.

The total eclipse of next December will last so short a time that, if possible, no part of that time should be wasted through a misapprehension of the nature of the phenomena to be observed. On this account I cannot but think it would be a matter to be much regretted that mistaken views should be promulgated respecting the corona, supposing it to be possible,—which I take

* *Monthly Notices*, for 1852, p. 183.
† *Journal of the Franklin Institute*, for 1869, p. 152.

to be the case,—to form just views from the evidence already in our hands.

The principal object of the observations to be made next December will be to ascertain the characteristics of the solar corona. Observers will certainly be able to work much more effectually if they know beforehand the general nature of the phenomenon, for they will thus be guided not only in the selection of modes of observations, but also by knowing what points it is most important they should attend to.

I think it so essential to avoid raising unnecessary doubts, that I would not venture to express the opinion that the corona is wholly a solar appendage if I had not given the matter very careful consideration, and found the evidence overwhelmingly strong in favour of this view.

It is hardly necessary to discuss the theory that the corona is due to the diffraction of solar rays which pass near the Moon's edge, because that theory has been thoroughly disposed of by Brewster's arguments. Nor need we consider La Hire's theory that the phenomenon is due to the reflection of the solar rays from the irregularities of the Moon's surface, as it is obviously inconsistent with the observed peculiarities of the corona.

But a theory has recently been put forward that the corona is simply due to the glare of the terrestrial atmosphere, and this theory has been adopted by astronomers of standing. I hold it to be important, therefore, that this theory should be subject to careful scrutiny, as undoubtedly, if it be erroneous, much mischief may be done to the cause of scientific progress by its promulgation at the present conjuncture.

The first and most obvious evidence against this theory is the fact that the Moon is projected as a dark disk on the bright background (so to speak) of the corona. The theory requires that the corona should, in fact, not be a background, but a foreground; and one might naturally inquire how the Moon which is beyond the Earth's atmosphere should come to be apparently projected upon the supposed glare of that atmosphere.

But though this circumstance is in itself decisive of the matter at issue, let us turn to less obvious considerations. As a matter of fact, we know that light reaches the eye along lines tend ng from the neighbourhood of the eclipsed Sun. Let us inquire whether in those directions there is illuminated air; if not, optical considerations will force us to regard the source of light as beyond the air.

The eclipse of next December is not a favourable one for my argument; but it will be more interesting, and perhaps more useful, to consider it than any other.

In fig. 1, let A represent the position of an observer on the line of central eclipse, somewhere in the south of Spain. At such a station the eclipsed Sun will be almost 30 degrees above the horizon; and I find from a valuable paper which Mr. Hind has been good enough to forward to me, that the shadow-cone will

be about 50 miles across, where it reaches the Earth. Obviously, then, the shadow on the Earth will be an ellipse whose major axis will be about 100 miles, its minor about 50 miles in length. Let A S then be drawn inclined at an angle of about 30° to H A H′, the horizon line at A, in a vertical plane through the Sun; and, having A *c*, A *c′* each to represent a space of 50 miles, let *c m* and *c′ m′* be drawn, each inclined about 16′ to A *c*,

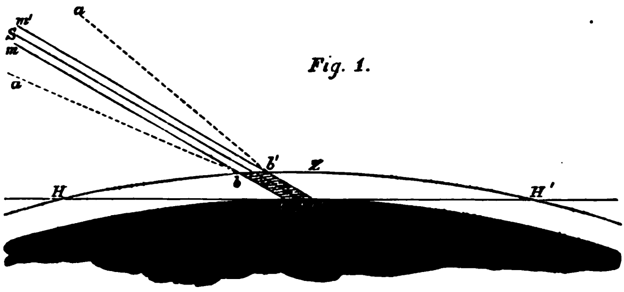


Fig. 1.

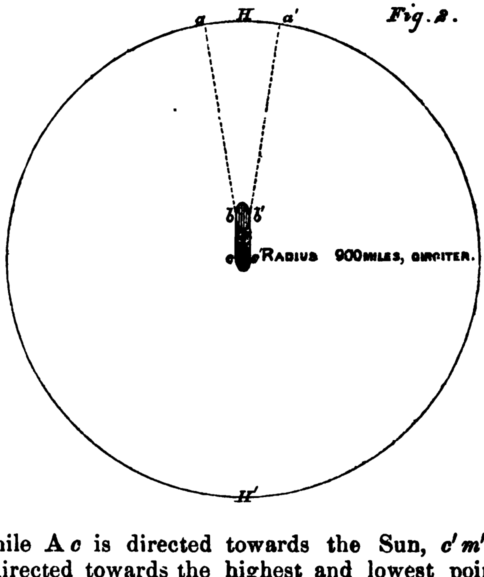


Fig. 2.

so that while A *c* is directed towards the Sun, *c′ m′* and *c m* would be directed towards the highest and lowest points of the Moon's limb. Then *m′ c′ m c* is a vertical section of the Moon's shadow.

Now we do not know the height of the terrestrial atmosphere, but we may confidently believe that no air above the height of *100 miles* can reflect any appreciable amount of solar light to

us.* Let us therefore take A Z to represent 100 miles; then H Z H′ will represent limits of the light-reflecting air, where H H′ is about 18 times as great as A Z. The portion of the atmosphere above the horizon-plane of the observer will therefore be of the figure produced by the revolution of H Z H′, about the vertical axis A Z. It will be, in fact, a plano-convex lens.

Let *c m* and *c′ m′* meet H Z H′ in *b* and *b′*; then the portion *b c c′ b′* will be in the Moon's shadow. (The effects of refraction are obviously insignificant.) The only light which can reach this part of the atmosphere is that from the chromosphere (to use a convenient but unsatisfactory name) and the coloured prominences, or from the earth and surrounding illuminated air. Towards *b* and *b′* the observer will recognise the first faint traces of directly illuminated atmosphere, and the light will gradually increase above *b′* and below *b* (more rapidly in the latter case than in the former). By a careful construction (a method quite exact enough for such an inquiry as the present), I make the the angle S A *a* about 6°, and the angle S A *a′* about 9°.

This, however, refers to only one section of the shadow-cone. To determine (roughly) the extent of illuminated atmosphere in a horizontal direction, we have only to consider the air-lens H Z H′ as supposed to be viewed from above. In fig. 2, *c c′* represents the actual shadow on the earth; *b b′* the intersection of the shadow-cone with the limits of our hypothetical envelope 100 miles high. Thus *b b′ c′c*, fig. 2, represents simply a vertical view of the portion *b c c′ b′* of the shadow-cone in fig. 1. Lines *b a*, *b′ a′*, drawn from the centre of the ellipse *c c′*, touching the ellipse *b b′*, give approximately the angular width of that part of the heavens within which no atmosphere directly illuminated by the Sun can be visible. I find from a careful construction that *a b* and *a′ b′* would include an angle of about 14½°.

Thus we obtain a nearly circular region (in which the Sun is eccentrically situated), having a horizontal diameter of about 14½°, and a vertical one of about 15°, within which there is not any light whatever from directly illuminated air. The Sun would be about 6° from the lowest point of this dark region.

With regard to the light from the prominences and the chromosphere, upon the air within this region, we know that it cannot suffice to light up the air with any strong, if even with

* Bravais, from a discussion of Lambert's observations of the crepuscular curve, deduced a height of nearly 100 miles. His own observations, made from the summit of the Faulhorn, gave a height of about 66 miles. Neither estimate refers to the actual limits of the atmosphere however. Dr. Balfour Stewart considers that perhaps the best means of judging on this point would be by observations made on the aurora. From such observations made in 1819, Dalton estimated the extreme height of the auroral light at 102 miles; Sir John Herschel estimated the height of an auroral arch seen on March 9, 1861, at 83 miles (undoubtedly the aurora is often seen much lower). The limits of air capable of reflecting light, must certainly lie much below the actual limits of the terrestrial atmosphere.

any appreciable glow; because we know how small a relation ordinary atmospheric glare bears to direct solar light, and the glare due to the chromosphere and prominences would bear a similar relation to the direct light from those sources. But further, whatever light came in this way, would obviously illumine the outer parts of the shadow-frustum $b\,b'\,c'\,c$ more strongly than the parts near the axial line A S. Hence a faint diffused light diminishing *towards* the neighbourhood of the Moon should result.

As regards the illumination of our shadow-frustum by light derived from the neighbouring illuminated atmosphere and from the Earth, it is only necessary to remark that even when there is no eclipse the light thus falling on such a region as $b\,b'\,c'\,c$ would be small; but that while a total eclipse is in progress all the parts near the shadow-cone are in nearly total eclipse, and not any part of the whole region H Z H′ is illuminated by so much as half the solar disk. Further, the light derived from this source, like that derived from the prominences and chromosphere, should diminish towards the neighbourhood of the Moon's disk, instead of increasing as the coronal light does. Also, the light from all these sources should extend over the Moon's disk, since it would illuminate the air between the observer and the Moon's body.

It follows, then, that so far from giving an account of the corona, atmospheric glare gives us a dark region round the eclipsed Sun, and a gradual increase of light with distance from him.

Within this dark space the disk of the Moon, illuminated by the Earth with about thirteen times as much light as the new Moon sends to us, ought to be conspicuous by its relative brightness.

Now, though the reasoning here deals with relations so simple that a mistake can hardly arise, yet there are certain tests to which these conclusions may be submitted before we proceed.

It is clear from fig. 1, that *before* the limits of the total shadow reached A there *should* be atmospheric glare towards the Sun, and further that this glare should at first wholly cover the Moon, and rapidly sweeping across her disk, just before totality, should pass away from her neighbourhood with undiminished velocity. It would be difficult to detect such a phenomenon by ordinary observation; though, as I shall presently show, not impracticable. But supposing a photograph could be taken an instant before totality, we might catch the glare while in the act of crossing the Moon's disk. Now this could only be managed by a miracle of dexterity; but by a miracle of good fortune, it *has* been managed already. The first photograph of Lieut.-Col. Tennant's admirable series was taken an instant before totality commenced; and *there we have the glare just about to leave the Moon's disk,*

Fig. 3.

but still trenching most obviously upon it. Fig. 3 represents the feature here dealt with. The light here is true atmospheric glare, and we see that, as might have been thought obvious, it is not limited by the Moon's disk which lies so far beyond the limits of the air. Then also we notice another important point. The edge of the glare is obviously travelling much faster than the Moon; for while the Moon proceeds to obliterate the last remaining point of the Sun's disk, the glare traverses the much wider distances separating its inner edge from the Moon's limb. Clearly this velocity would carry the glare clean away from the Moon, as the above reasoning shows should be the case

Again, it will be obvious, from a study of figs. 1 and 2, that during an annular eclipse, at the moment when the shadow-cone is pointing directly, or almost directly, towards the observer, the centre of the Moon's disk ought to be much darker than the edge. Now in the 10th volume of our *Memoirs*, Mr. Baily states that, while observing the eclipse of 1836, he noticed, on looking at the Moon through a telescope during the annularity, that "the circumference was tinged with a reddish purple colour which extended over the whole disk, but increased in density of colour according to the proximity to the centre, so as to be in that part nearly black." It is obvious that this appearance could last but a few seconds, since the moment the axis of the shadow-cone was turned appreciably away from the observer (and the vertex of the cone travels fully 20 miles per minute), he would be looking through the cone's *sides*. The following passage from Klein's *Sonnensystem* describes the whole phenomenon precisely in accordance with this view: "Bei der ringförmigen Finsterniss, am 30 October, 1864, sah Mouchez zu San Catharina in Brasilien, im Augenblicke als die Scheiben von Sonne und Mond concentrisch waren, das Centrum des Mondes völlig dunkel, aber von hieraus gegen dem Rand nahm die Helligkeit regelmässig zu und letzterer erschien heller, oder doch wenigstens ebenso hell, als das aschgraue Licht der Mondsichel, kurze Zeit vor oder nach dem Neumonde. Die ganze Erscheinung verschwand und die Mondscheibe war gleichförmig dunkel, als der leuchtende Ring gerissen und die Mitte der Finsterniss vorüber war."

Taking the corona to be a solar appendage, it is clear that even in total eclipses a somewhat similar appearance might be looked for, the outer parts of the Moon's disk during central totality seeming brighter than the centre, because the atmosphere between us and those parts would be more fully lighted up by the corona. I find, accordingly, that M. Tissel, observing the

total eclipse of 1733 at Skepshat, in Sweden, saw the Moon's surface brighter at the margin, and black towards the middle. We see from this most clearly that the atmospheric glare in this region is very much fainter than the corona, for except on a very close examination the Moon's disk, though the glare appears over a part of it during the totality, seems absolutely black, and is so rendered in photographs.

But further, if the views expressed above are correct, it ought to be possible under favourable circumstances to see the Moon's face by reflected Earth-light. I find that Bigerus Vassenius, during the remarkable total eclipse of 1733, using a telescope of 21 feet focal length, perceived the principal spots on the Moon during the total obscuration (*Phil. Trans.* 1733, p. 135). Ferrer also saw the spots on the Moon's surface very plainly during the total solar eclipse of 1806.

Yet again, if the apparent blackness of the Moon's disk results from the fact that the coronal light is beyond the Moon, and so forms the background on which she is projected,* two phenomena might be expected to be visible under favourable circumstances. First, the entire outline of the Moon's disk ought to be visible in partial eclipses, or before and after totality; and secondly, the corona ought to be visible at such times, and also, during annular eclipses. I find that the former phenomenon which corresponds in reality to the visibility of the corona (since were there no corona the Moon's limb could not appear dark where it crossed the Sun), has been frequently noticed; it has, in fact, been as often recognised as looked for. The visibility of the corona, when the Sun is not totally eclipsed, has also been so frequently recognised that it is hardly worth while to mention instances in point.† But I may quote, as very remarkable, the the fact that in 1860 Father Secchi saw the corona *for forty seconds after totality was past.* Another remarkable instance is that recorded by M. Edstrom, in the case of the eclipse of 1733, when the unequal radiations of the corona were observed to remain unchanged in position, as they gradually faded out of view with the increasing solar light.

It is further obvious, that if the corona be a solar appendage, one could expect it to appear concentric with the Moon only at

* The fact that the disk of *Venus* appears blacker than the surrounding sky when she is in superior conjunction, can only be explained by supposing there is some light beyond *Venus*. What can that light be but a solar appendage?

† Arago has founded on the visibility of the corona while a portion of the Sun is yet uneclipsed, a calculation of the ratio which the coronal light exceeds that of the atmospheric glare *then* undoubtedly present. That the corona is brighter than the atmospheric glare caused by a portion of the direct solar light undoubtedly follows from the visibility of the corona under such circumstances; but Arago's mode of treating the problem is not exact. He makes the atmospheric glare proportional to the portion of the solar disk visible at the moment. In reality, this proportion does not hold, for the upper regions of the air are illuminated by much more of the Sun's disk at such a time. In fact, the problem is one of much greater complexity than Arago seems to have imagined.

the moment of central eclipse. Now I find numerous instances in which it has been stated that quite obviously the brightest part of the corona was first on the side the Moon had just covered before totality, and lastly on the side she was just about to leave uncovered. I also find several statements (one or two very positive) that the corona was centrally disposed round the *Moon* throughout the totality. I would remark on this, that observations of the former kind, besides being more numerous, are severally more effective than observations of the latter kind. For the former refer to the recognition of a phenomenon and afford positive evidence; the latter merely assert the non-recognition of the phenomenon, and supply therefore only negative evidence. The former describe a peculiarity which attracted the notice of observers; the latter may be taken quite as well to indicate a want of skill in observation as the non-appearance of the particular phenomenon in question. All the positive evidence is therefore here also in favour of the view that the corona is a solar appendage.*

It remains that I should touch on other evidence we have of the existence of a solar appendage adequate to produce the observed appearances.

And first let us consider the zodiacal light. We know that even in our latitudes this phenomenon often exhibits a remarkable degree of luminosity towards the horizon and near the core of the gleam (so to speak). But in tropical countries the brightness of the zodiacal light is much more striking, and is seen to grow visibly greater in the Sun's neighbourhood. At heights of from 8000 to 12,000 feet in tropical climates, says Humboldt, the zodiacal light is seen of a brightness exceeding that of the Milky Way between *Aquila* and *Cygnus*. And obviously if we could trace the zodiacal light up to the solar limb, we should see it shining with a glory far exceeding that which it shows even in tropical countries. For we know that the brightest part there seen belongs still but to the outskirts of the object.†

* The centricity of the corona concerns, however, the question whether the corona is a solar or a lunar appendage; since the atmospheric glare should shift even much more obviously with respect to the Moon than a solar appendage would seem to do. No one now holds that the corona is a lunar appendage.

† I ought, perhaps, to show reason for regarding the zodiacal light as a solar appendage, notwithstanding Dr. Balfour Stewart's recent suggestion that it may be a terrestrial phenomenon. But in reality there can be no doubt whatever that the zodiacal light cannot be a phenomenon associated in any way with our atmosphere. Doubtless Dr. Stewart, whose mathematical attainments are well known, must have directed his attention too exclusively to the physical requirements of his theory, or he would not have overlooked obvious mathematical objections against it. The portion of the return-trade region above the horizon of any place is clearly a lamina shaped like a watch glass (slightly convex, see fig. 1), and the whole of this should be illuminated by electrical discharges excited in the way he suggests. We may, in fact, see in this an explanation of the familiar phosphorescence seen sometimes to cover the whole heavens (which gives the same spectrum as the aurora and zodiacal light), and even though at times, or even commonly, only a portion of this lamina should

Hence we should expect to find precisely such a glow of light round the Sun in total solar eclipses as we actually do see.

Again, from what we now know respecting meteors, we may derive abundant evidence in favour of the view that countless myriads of these bodies must always lie in the Sun's neighbourhood. For though, while the meteoric orbits were supposed circular, there was nothing very surprising in the fact that the Earth encountered so many as fifty-six meteor systems (because the zone of such systems seemed to lie close by the Earth's orbit), yet now we know how eccentric the meteoric orbits really are, we recognise the fact that antecedent probabilities would be wholly against the Earth's encountering even one such system, were there not many millions of them. And since she encounters fifty-six at least, we conclude that there must be millions on millions of such systems having their perihelion within the Earth's orbit. These uncounted systems ought to become visible during a total eclipse, since their dispersed members would lie in all directions round the Sun. Those meteoric flights also which were near him (and many must pass very near to him) would shine with a light whose brilliancy would go far to make up for the extreme relative minuteness of the individual meteors. Since near the Sun's disk the line of sight would be directed through a range of many millions of miles over which such meteors must be freely distributed, while along some 200 millions of miles in this direction meteors must be scattered, though more or less sparsely, one can recognise the reason of the brightness of the corona near the chromosphere.

Further, we know from the researches of Leverrier that there must exist continually in the Sun's neighbourhood a quantity of matter sufficiently important to affect the motion of the perihelion of *Mercury*. A few relatively considerable planets (as large say as the asteroids) might effect the observed changes; but far more

be so illuminated, no reason can be shown why that portion should always be an inclined tongue-shaped slip, as it should be to account for the zodiacal light in our latitudes. It is hardly necessary, however, to point out to the astronomical reader that a light which exhibits no parallactic displacement, which varies in position for different latitudes, according to the laws affecting the celestial bodies, which rises and sets according to the same laws, and which lastly effects the neighbourhood of the elliptic, cannot by any possibility belong to the Earth's atmosphere. The zodiacal light *might* be explained as due to a ring of matter surrounding the Earth, at a distance nearly equalling the Moon's, and travelling (as such a ring would) nearly in the plane of the ecliptic. Such an explanation was indeed put forward in 1856 by Prof. Heis. But the phenomena of the zodiacal light are much better explained by the theory that it is due to a solar appendage, even if we admit that the light sometimes extends from the eastern to the western horizon. But while Heis' theory, with overwhelming probabilities against it, has some points in its favour, the theory that the zodiacal light is an atmospheric phenomenon, is absolutely untenable. If anything would render the theory more strikingly opposed to observation than it is, it would be those occasional peculiarities of the zodiacal light which have been thought by some to favour the theory. These peculiarities simply add new difficulties to others already overwhelming.

probably a multitude of minute bodies may be held to be in question. Now the constant presence of meteors in the Sun's neighbourhood would produce the observed results, even though the individual meteors might remain but a brief time in the Sun's neighbourhood, to pass away presently on orbits whose aphelia might lie far beyond the orbits of the major planets.

Further, Mr. Baxendell has shown that certain peculiarities of magnetic and thermal change seem to point very decisively to the existence of a solar appendage holding the position which the corona, regarded as solar, seems to occupy. I have recently had the pleasure of discussing with him many of the relations considered above, and I find that there is nothing in his valuable meteorological researches which opposes itself to that particular view of the corona which I have advocated above, while his main result (which I hold to be of extreme importance) supplies an obvious argument in favour of that view.

Lastly, there are certain peculiarities in the aspect of the corona which seem only explicable on the theory above enunciated. Such are those radiations which are not at right angles to the Sun's limb; the phenomenon of loops of light in the corona with their concavity directed towards the Sun; the strange appearance resembling a hank of thread in disorder, seen by Arago in 1842; and other peculiarities too numerous to specify.

I know not of any phenomena which oppose themselves to the view here put forward, though I have carefully sought for such.

The spectroscopic analysis of the corona has not hitherto been altogether satisfactory, so that it may hardly be well to lay much stress upon it. It accords very satisfactorily, however, with the above theory. There would be a large quantity of reflected solar light in the corona, but there would also be much light from incandescent meteors, since those which came within a million or so of miles of the Sun would undoubtedly be raised to a white heat. Some of the meteors would, in all probability, be vapourised, and so bright lines might be seen in the coronal spectrum, as appears to have been the case during the total eclipse of last August. The observed association between meteors and comets suggests obvious considerations in explanation of the peculiarities said to characterise the spectrum of the corona. If the Great Comet of 1843 which passed within 65,000 miles of the Sun, has, like Tempel's comet, a train of meteoric bodies following in its track, these must be vapourised in the Sun's neighbourhood.

The contradictory evidence afforded by the polariscope is also obviously accounted for by the theory I have here advocated, even if it may not be said of itself, to force upon us the belief that the light of the corona is of that mixed kind which could scarcely result but in the way specified in that theory.

It would be desirable that measures should be adopted to insure the application of effective modes of observation during the very brief interval of total obscuration. I think this Society might with advantage appoint a committee to consider whether novel appliances and methods might not be employed to good purpose. The points I now proceed to touch on are so simple that some apology may, perhaps, be needed for bringing them under the notice of the Society; but if they should lead practised observers to make really important suggestions, my purpose will have been fulfilled.

In the first place, I would remark that observations specially directed to prove that the corona is a solar appendage would, in my opinion, be a complete waste of time and skill. It would be a misfortune — nay, it would even be in a sense discreditable — to astronomy, if the attention of observers should be directed to the solution of a question which has been practically solved during former eclipses. Unless the most obvious considerations of mathematics and optics are to be entirely neglected, the position of the corona as a solar appendage must be regarded as established, and all observations made with the object of confirming or disproving the matter, as simply futile.

But if we must travel over old paths, in order to establish that which is already evident, there are a few modes of observation which may be suggested as likely to give significant, however unnecessary, evidence.

If an observer were to confine his whole attention to the lunar disk during the eclipse, having a telescope with well-adjusted clock movement, and a field somewhat less than that of the full Moon, he would be able to recognise the following striking proofs of the real way in which the glare of the atmosphere varies during an eclipse. He would see, as the total phase approached, the atmospheric glare over the Moon's face gradually diminishing, and then what remained of actual glare from direct solar rays sweeping rapidly across the face of the Moon and leaving her disk relatively dark. But in a few moments the observer would be able (in favourable atmospheric circumstances) to recognise the spots on the lunar surface.

If an observer were to limit his attention to the Moon's disk during totality, keeping his eyes in darkness until the commencement of totality was announced by those around him, he would be certainly able to see the lunar spots, unless atmospheric conditions were very unfavourable indeed.

Attention might be directed to the shape and motions of the dark region of the sky surrounding the corona; and such observations would not be so complete a waste of time as those last considered, since it is evident that important information might be gathered from them respecting the height of the atmosphere. Such information would be in many respects more trustworthy than that which has been derived from the position, shape, and motions of the crepuscular curve.

But a mode of observation which I would advocate with great earnestness, is the simple application of telescopic power to determine, if possible, the structure of the corona. I have no doubt that this structure is continually changing; but most valuable information might be gained from a careful study of the position of the coronal beams at the time, and of those singularly complex hanks and streamers which have been already noticed by astronomers. The use of a telescope of low magnifying power, but first-rate definition, a comet eye-piece being employed, would be desirable in thus studying the corona. The telescope should be accurately driven by clock-work, and a dark iris-disk, if I may so describe an arrangement which would be the converse of an iris diaphragm, might be employed with advantage to hide the light of the prominences and chromosphere. If the field of view were several degrees in diameter, and the dark disk at the beginning of totality concealed a circular space extending a degree or so beyond the eclipsed Sun, the observer might first examine with great advantage the outer parts of the corona, and gradually extend his scrutiny to the very neighbourhood of the prominences. Supposing his eyes had been kept in darkness before totality began, he would be able to gain such an insight into the real structure of the corona as has never yet been obtained by astronomers.

As regards the spectroscopic and polariscopic analysis of the corona I shall say little. It would obviously be most desirable that Mr. Huggins, Mr. Lockyer, and those astronomers whose attention has been practically directed to researches of this sort, should give careful consideration to the question how the short interval of totality may best be employed, and that they should make their views public as early as possible. To one point, however, I shall venture to direct the attention of observers. It seems to me most important that every observer proposing to take part in applying such delicate light-tests to the corona, should prepare for the observations he is to make by keeping his eyes in darkness as nearly complete as possible for some time before totality commences; and further, where different parts of the corona are to be examined, the fainter parts should be first dealt with.

If the search for an intra-Mercurial planet is to be renewed with any chance of success, there can be little doubt that the telescopist must keep the corona, or at least its brightest portions, out of the field of view. A telescope specially constructed for the purpose, having a motion carrying the tube conically round a mean position might easily be devised; and with such an instrument one might conveniently sweep the Sun's neighbourhood all round the limits of the corona, for *Vulcan* and perhaps a train of attendant *Cyclopes*. But a telescope of low power, with a comet eye-piece, and a diaphragm concealing the brighter part of the corona, would probably be quite as effective.

For this class of observation, also, it would be very advan-

tageous that the eyes should be kept in darkness for some time before totality commenced.

Are observers to be found who, supposing the circumstances of the coming eclipse to be favourable, will be ready to forego the opportunity of witnessing one of the grandest of all natural phenomena, of watching the gathering shadows, of beholding the wonderful transformation of the face of nature, the weird and unearthly aspect of all things round them, and the strange beauty of the solar corona of glory, in order that they may devote all their observing energies during two short minutes to important, but severally uninteresting, phenomena? We know that, so far as the period of totality is concerned, such a sacrifice has already been made by De La Rue and Tennant, by Secchi, Janssen, Lieut. Herschel, Young, and a number of other lovers of science; but no observer has yet foregone the whole spectacle of a total eclipse for the sake of the dull, dry details of scientific observation.

Ephemeris of the Satellites of Uranus. By A. Marth, Esq.

Angles of Position at 8^h Greenwich Mean Time.

1870.	Ariel.	Umbriel.	Titania.	Oberon.
	°	°	°	°
Feb. 18	152	70	317	161
19	3	346	272	137
20	217	258	223	109
21	79	172	185	77
22	303	85	150	46
23	157	357	108	19
24	8	273	60	357
25	223	182	17	334
26	87	100	342	309
27	309	7	305	281
28	162	288	257	248
Mar. 1	13	193	211	218
2	229	114	174	193
3	93	18	139	170
4	314	301	95	148
5	167	204	46	122
6	18	128	7	92
7	236	30	332	60
8	100	314	292	31
9	320	216	242	7
10	172	140	200	345
11	24	42	165	321

1870.	Ariel.	Umbriel.	Titania.	Oberon.
	°	°	°	°
12	243	325	127	294
13	107	229	80	263
14	325	151	33	232
15	176	56	357	204
16	29	336	321	181
17	250	243	277	159
18	114	162	229	135
19	330	71	189	106
20	181	347	154	75
21	35	257	114	44
22	256	172	65	18
23	120	86	22	355
24	335	357	346	333
25	186	274	310	308
26	41	182	263	278
27	264	101	216	246
28	126	8	179	216
29	340	288	144	192
30	191	193	100	169
31	47	115	51	146
Apr. 1	271	19	11	120
2	132	302	336	90
3	345	205	297	58
4	196	128	249	29
5	53	31	204	5
6	278	314	169	343
7	137	217	132	320
8	350	141	86	293

The Apparent Distances vary between the Limits

Ariel	15"	and	12"
Umbriel	21		16
Titania	35		27
Oberon	46		36

The Zodiacal Light. By Capt. Noble.

On the evening of March 3rd, from $7^h\ 40^m$ to 8^h, L.M.T., this phenomenon was much brighter and more conspicuous than I have ever seen it before, surpassing in vividness that part of the Milky Way running through *Cepheus* and *Cygnus*, which was, of course, favourably situated for comparison. It is stated in most of the popular books on Astronomy, that the axis of the

Zodiacal Light nearly coincides with the ecliptic; but, on the occasion to which I refer, it certainly trended considerably to the right of it (as viewed with the naked eye). The boundaries of the light were not well defined. It involved α, β, and γ *Arietis*, and extended a little way upwards towards *Andromeda*. The *Pleiades* were markedly to the left of it, and separated from it by an unmistakable gap of dark sky. Allowing for the difficulty of estimating accurately the axis of a figure, which requires averted vision to reduce to anything like shape; and speaking roughly, I should say that such axis was inclined some 20° to the ecliptic, according to the best estimation that I could form.

Forest Lodge, Maresfield, Sussex,
March 10th, 1870.

Occultation of a Star by the Moon. By Capt. Noble.

Thursday, February 10th, 1870, *m Tauri* disappeared instantaneously at the Moon's dark limb at $6^h\ 33^m\ 30^s{\cdot}2$, L.S.T.= $9^h\ 10^m\ 33^s{\cdot}1$, L.M.T. and reappeared at the bright limb somewhere about $7^h\ 12^m\ 45^s$, L.S.T.=$9^h\ 49^m\ 41^s{\cdot}5$, L.M.T. This was an unsatisfactory observation, as no determination was made of the errors of adjustment of the Transit when the time was taken; and, in addition, the reappearance was badly seen, the star being clear of the Moon's limb when first glimpsed. Power, 255, adjusted on the star.

Observations of Venus near her inferior Conjunction. By Capt. Noble.

The day on which the Planet was actually in inferior conjunction was densely cloudy here; but on the previous one, Tuesday, February 22nd, I observed her at $2^h\ 10^m$, L.M.T. when she was within $24^h\ 14^m$ of such conjunction. She presented the appearance of an exquisitely delicate thread of light, the line joining the cusps being a chord less than a diameter: in other words, the hair-like luminous line did not extend round a semicircle. A defect in the driving-clock of my Equatoreal precluded me from making any micrometrical measurements, however, and this must be my excuse for speaking thus vaguely. Constricting the field of view of a Huyghenian eye-piece magnifying 154 times by means of a card diaphragm pierced with a central needle line, I could see, plainly enough, the dark body of the planet. The sky was somewhat hazy, and I could not trace the dark limb *quite* round; but its difference of tint from that of the surrounding sky was evident the instant *Venus* was regarded. I employed powers of 74, 115, and 154. Vision was most satisfactory with the latter.

Note on further changes in the Coloured Belt of Jupiter.
By John Browning, Esq.

The coloured belt on Jupiter to which I have recently had the honour of calling the attention of the Society, has undergone many changes both in form and hue since I last described the appearance of the planet.

The ochreish-yellow colour is rather fainter, and of a duskier hue, and it is confined to the northern part of the equatorial belt, instead of covering the whole of it as was formerly the case.

It would, perhaps, be more correct to say that there are two belts near the equator, one to the north, of a faint dusky yellow, not dark, but very dim, and one to the south, which is pure white. This belt is by far the brightest portion of the planet's disk, and it is the only portion of the disk which is colourless.

The drawing I have the honour to exhibit represents pretty faithfully the appearance of the planet on January 31st, at 9.30, G.M.T.

March 10th, 9.30 P.M.—Observed *Jupiter* with reflector, 12-in. aperture, and power 140, achromatic eye-piece. The planet being very low, I could not use a higher power with advantage. The tawny yellow colour now again extends over the whole of the equatorial belt, which is broader than I have ever seen it before. This belt has a very dark band on the south, and a narrower dark band on the north; beyond each of these there is a very brilliant white belt. These two belts are the brightest portions of the planet's disk. As this striking outbreak of colour appears to be on the increase, it is very desirable that those observers who have a good western view should observe the planet, and record their observations at every possible opportunity, using the Astronomer Royal's new correcting eye-piece for the purpose.

Minor Planet (109) *Felicitas.*

Discovered 9th October, 1869, by Dr. C. H. F. Peters, at Clinton, N. Y.

The following Elements, calculated by him from observations up to 9th December, are given with an Ephemeris, *Ast. Nach.* No. 1788:—

Epoch, 1870, Jan. 0·0 Berlin M.T.

$$
\begin{aligned}
M_0 &= 357^\circ\ 33'\ 46''\cdot 82 \\
\varpi &= 55\ 53\ \ 6\cdot 07 + 50''\cdot 24\, t \\
\Omega &= 4\ 57\ 12\cdot 20 + 47\ \cdot 59\, t \\
\iota &= 8\ \ 3\ 17\cdot 56 + 0\ \cdot 46\, t \\
\varphi &= 17\ 27\ \ 5\cdot 47 \\
\mu &= 801''\cdot 820 \\
\log a &= 0\cdot 4306198
\end{aligned}
$$

Where t is reckoned in years from 1850·0.

ERRATA.

Page 43, line 27, *for* Fareham, *read* Farnham.

Page 134, "Dome for Sale," *for* 25 feet diameter, *read* 15 feet diameter.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXX. *April* 8, 1870. No. 6.

William Lassell, Esq., President, in the Chair.

Lieut. Abney, Royal Engineers,
R. Abbay, Esq. Wadham College,
W. Thirlwall Bayne, Esq., 34 Jermyn Street,
C. J. Lambert, Esq., Pembroke College, Cambridge,
Dr. Riches, 1 Lansdowne Crescent, Bath, and
Geo. M. Seabroke, Esq., Rugby,
were balloted for and duly elected Fellows of the Society.

The following Circular has been, by direction of the Council, issued to the Fellows of the Society:—

Sir,—We are instructed to communicate to you the following resolution, which was passed at a Committee of the Council held yesterday, April 8th,

Resolved—"That the Fellows be informed that there is a possibility of the Government providing means of transit to and from stations on the Mediterranean for about sixty observers, who may be willing to take part in the Observation of the Total Eclipse of December 22, 1870; and that persons willing to undertake a portion of the Observation, on a plan to be arranged by the Council, be invited to send their names to the Secretaries, and also to state the branch of observation which they would be prepared, or prefer, to undertake, and the instruments they would be willing to contribute."

It is desirable that the names of those who are willing to take part in the observation of the Eclipse should be sent in, if possible, before the next Meeting of Council, May 13.

We have the honour to remain, Sir, yours faithfully,

William Huggins, } *Hon. Secs.*
E. J. Stone, }

Royal Astronomical Society, April 9, 1870.

On the Orbit of the Comet of 1683. By Mr. W. E. Plummer, of Mr. Bishop's Observatory, Twickenham. (Note by Mr. Hind.)

The comet of 1683 was first seen by Flamsteed on the 23rd of July, and was assiduously observed at Greenwich until the 5th of September. The original observations are printed in the first volume of the *Historia Cælestis.* Hevelius observed the comet at Dantzic from the 30th of July to the 4th of September, and the particulars of his observations appear in his *Annus Climactericus.*

Halley calculated a parabolic orbit for this comet from Flamsteed's observations, which was published in his *Synopsis of Cometary Astronomy,* and is as follows :—

Perihelion Passage, 1683, July 13, at $3^h\ 0^m$, G.M.T.

Longitude of Perihelion	85° 29′ 30″
Longitude of Ascending Node ..	173 23 0
Inclination of the Orbit	83 11 0
Logarithm of Perihelion distance ..	9·748343

Motion Retrograde.

In the fifth volume of the *Astronomische Nachrichten* (No. 117) Clausen (the present director of the Observatory at Dorpat) has given elliptic elements for this comet, founded as he states upon a new reduction of Flamsteed's observations. These elements are as follow :—

Perihelion Passage, 1683, July 12·73236, M.T. at Paris.

Longitude of Perihelion	86° 31′ 15″	Mean Equinox,
Longitude of Ascending Node ..	173 17 48	1683·0.
Inclination of the Orbit	83 47 46	
Logarithm of Perihelion distance ..	9·7430148	
Eccentricity	0·9832470	

Motion Retrograde.

The period of revolution in this ellipse is 189·8 years, which would bring the comet to perihelion again in 1873.

Mr. Bishop having, on my suggestion, requested Mr. Plummer, assistant in his observatory, to make a new and rigorous reduction of Flamsteed's observations, an accurate Ephemeris of the comet was first calculated for the whole interval between the extreme observations, the Sun's longitudes and the radii-vectores of the Earth, being deduced from the Solar Tables of Leverrier. The places of the comparison stars were carried back, from the Greenwich Catalogue for 1860, undoubtedly the highest existing

authority. The observed distances of the comet from stars were carefully corrected for refraction and parallax, a mean value of the refraction being employed, and the distances in the computation of parallax inferred from Clausen's ellipse. A comparison of the observations thus rigorously reduced with Clausen's orbit was then made, and it was at once evident that an orbit might be found to represent the observations much more satisfactorily than the ellipse in question. This Mr. Plummer proceeded to investigate, and the result is a parabolic orbit which accords as well with the whole course of observations as the limits of their errors will allow. The elements of this parabola are :—

Perihelion passage, 1683, July 13, 09068, Greenwich M.T.

Longitude of Perihelion	85 35 59·5	} Mean Equinox, 1683·0.	
Longitude of Ascending Node ..	173 24 39·7		
Inclination of the Orbit	83 13 14·7		
Logarithm of Perihelion distance ..	9·7478656		

Motion Retrograde.

The sums of the squares of the errors are :—

	R.A.	N.P.D.
In Clausen's ellipse ..	337278″	224845″
In Plummer's parabola ..	29089″	58394″

It would therefore appear that there is no reason to expect the return of the comet within the next few years, or to suppose that it is one of moderate period.

If I am not greatly mistaken, Clausen has merely reduced Flamsteed's observations on three days, and computed an orbit upon them by a general method, such as that given by Gauss in the *Theoria Motus*. The observations selected are almost indicated by the comparison with his elements.

I should add that the observations of Hevelius were likewise rigorously reduced, but being found greatly inferior to Flamsteed's in point of accuracy, it was considered that no advantage would be gained by employing them in the calculations for the orbit.

It is the intention of Mr. Bishop, to publish the details of Mr. Plummer's investigation in a separate form.

1870, *April* 7.

Studies on the frequency of Sun-spots, and on their connexion with the Magnetic Declination-variation. By Prof. Rudolf Wolf. (Translation.)

Denoting by r the Relative-number for the Sun-spot frequency introduced by me in the year 1850, by f the number of days with-

out spots, and by *b* the number of days of observation, I obtain for 1864–69 the following table:—

	1864.		1865.		1866.	
	r	*f* : *b*	*r*	*f* : *b*	*r*	*f* : *b*
January	57·5	0 : 31	48·3	0 : 30	33·3	0 : 31
February	47·2	0 : 29	44·8	0 : 27	39·4	1 : 28
March	67·3	0 : 31	40·7	2 : 28	27·2	0 : 29
April	30·0	2 : 29	32·5	1 : 30	18·9	2 : 30
May	40·9	0 : 31	37·5	2 : 31	15·0	6 : 31
June	58·3	0 : 30	36·3	2 : 30	18·3	3 : 30
July	57·2	0 : 31	29·7	2 : 31	10·2	9 : 31
August	57·9	2 : 31	40·3	0 : 31	14·0	5 : 31
September	30·5	1 : 30	22·9	7 : 30	8·0	13 : 30
October	35·5	0 : 31	18·5	10 : 31	14·6	5 : 31
November	59·1	0 : 28	24·7	4 : 27	9·3	16 : 30
December	24·1	1 : 24	13·3	9 : 28	1·6	26 : 30
Year	47·1	6 : 356	32·5	39 : 354	17·5	86 : 362

	1867.		1868.		1869.	
	r	*f* : *b*	*r*	*f* : *b*	*r*	*f* : *b*
January	0·0	29 : 29	12·2	13 : 25	72·4	0 : 27
February	0·8	26 : 28	16·4	5 : 28	72·4	1 : 24
March	10·8	11 : 31	28·7	3 : 30	65·3	0 : 28
April	5·8	20 : 30	39·4	0 : 30	46·5	1 : 30
May	3·3	24 : 31	30·3	3 : 31	115·8	0 : 30
June	1·6	26 : 30	34·7	2 : 30	120·4	0 : 30
July	5·3	18 : 31	32·2	10 : 31	65·1	1 : 31
August	5·9	19 : 31	38·6	0 : 31	93·2	0 : 30
September	10·6	15 : 30	52·6	1 : 30	88·5	0 : 30
October	14·2	13 : 31	60·5	0 : 29	62·4	0 : 30
November	10·3	9 : 30	67·9	0 : 21	85·7	0 : 24
December	27·5	6 : 24	68·4	0 : 29	122·1	0 : 25
Year	8·0	216 : 356	40·2	32 : 345	84·1	3 : 339

One recognises herein, at the first glance, the minimum of 1867 according with my Sun-spot period of $11\frac{1}{9}$ years, and from that date the rapidly increasing Sun-spot frequency. By the empirical formula which, since 1859, I proposed for various points in order from *r* to deduce the declination-variation *b*, I give here, only by way of example, the calculated formula 1852–61 for Christiania,—

$$v = 0'{\cdot}0413\ r + 4'{\cdot}921$$

This gives the following table, where *v* denotes the variations calculated by the formula, *v′* the values resulting from the actual

observations, communicated to me by Messrs. Mohn and Fearnley.

	1864.	1865.	1866.	1867.	1868.	1869.
v	6'·87	6'·26	5'·64	5'·25	6'·58	8'·39
v'	6·00	5·72	5·70	5·69	6·65	7·82

It is hardly necessary to remark that the constants of the formulæ may, by means of the whole series of the Christiania Observations, easily be altered so as to obtain a yet better agreement. I prefer, however, for the present not to make this change.

Zurich, March 28, 1870.

Observations of Lunar Eclipse, Jan. 17, 1870. By J. Tebbutt, jun.

The lunar eclipse of the 17th instant was remarkably well seen here. The Moon was overspread with very thin filmy cloud till about $11^h\ 43^m$, but the diminution of her brilliancy from that cause was very slight. She remained unclouded during the rest of the phenomenon. No decided defalcation of light was noticed on the eastern limb till $10^h\ 41^m$, but at $10^h\ 52^m$ the effects of the penumbra were very marked. The following are the local mean times of the different phases as near as they could be observed, it being a most difficult matter to fix the precise instants of the contacts owing to the ill-defined character of the shadow:—

	d	h	m	s
First contact with the shadow	17	11	1	19
Beginning of the total phase	..	12	0	29
End of the total phase	..	13	38	53
Last contact with the shadow	..	14	38	58

At $11^h\ 29^m$ the shadow assumed a light copper tint, except at its periphery, where it was of a very dark green. The copper tint, as seen in the telescope, appeared to extend even to the filmy cloud which lay along the Moon's eastern limb. At $11^h\ 43^m$, when the Moon shone unclouded, the details on the obscured portion of the lunar surface began to be perceptible in the telescope. These became gradually more distinct, and it was soon observed that the dark body of the Moon was surrounded by numerous telescopic stars, and that many occultations would occur during the total phase. Several of these phenomena were observed with tolerable accuracy; some of the stars, however, were too faint for accurate observation. The following occultations were recorded:—

Star.	Mag.	Phase.	Mean Time. h	m	s	Remarks.
a	7	Disappearance	11	59	24·7	Disappearance sudden.
b	8½	,,	12	49·4		(Approx.)
c	8	,,	13	2	19·3	Disappearances near upper limb: a little uncertain.
d	8	,,	13	5	0·2	
e	8	Reappearance	13	6	18·7	
a	7	,,	13	25	4·2	(Approx.)
f	7	Disappearance	13	36	24·2	Uncertain to a second, owing to increasing brightness of limb.

The noted time of the reappearance of the star *a*, owing to a temporary removal of the eye from the telescope, was probably two or three seconds late. The Moon's disk was of a copper hue throughout the total phase, and continued distinctly visible both to the naked eye and in the telescope. The southern limb was remarkably bright at the middle of the eclipse. The meridian transit of the first limb was pretty well observed, but the second limb was too faint. The copper and dark green tints were again observed after the total phase, that portion of the obscured surface next to the centre of the shadow being copper-tinted, and the outline of the shadow being very dark green. The telescopic observations during the eclipse were all made with my refractor of 3¼ inches aperture, and 48 inches focal length, furnished with a magnifying power of about 30.

Windsor, New South Wales, Jan. 26, 1870.

A few further Notes on the Floor of Plato.
By W. R. Birt, Esq.

In my communication on the floor of *Plato*, which was read at the Meeting of the Society in November last, I solicited attention to a feature presented by the spots, and which I ventured to term "degree of visibility." The paper had reference to twenty-five spots, the degree of visibility of sixteen being given. The entire number of observations to Sept. 27, 1869, was 238. Since that date the observations have been continued, and greater attention has been given to the subject, and as twelve months' observations are now completed, including a luni-solar year, the following results may not be uninteresting.

The number of spots which have been seen on the floor of *Plato* up to the present time is thirty-five, eight of which have been detected since Sept. 27, 1869. The number of observations since that date amounts to 531, being more than double the number (240) during the first six lunations of the year 1869, April to 1870 March inclusive. In the following table the "degree of visibility" of each spot is given for the first six lunations, *the last six* lunations, the increase or decrease of visibility of

those spots which are comparable, and the "degree of visibility" of each spot for the year, the number of observations being 771.

	April to Sept.		Oct. to March.			Year.	
No.	Obs.	Vis.	Obs.	Vis.	Diff.	Obs.	Vis.
0	..	..	5	·070	..	5	·048
1	33	1·000	71	1·000	..	104	1·000
2	1	·030	6	·084	+·054	7	·067
3	24	·727	68	·958	+·231	92	·885
4	28	·848	62	·873	+·025	90	·865
5	20	·606	30	·423	−·183	50	·481
6	8	·242	24	·338	+·096	32	·308
7	8	·242	8	·113	−·129	16	·154
8	..	..	3	·042	..	3	·029
9	5	·151	20	·282	+·131	25	·240
10	6	·182	5	·070	−·112	11	·106
11	1	·030	14	·197	+·167	15	·144
12	3	·091	1	·014	−·077	4	·038
13	16	·485	12	·169	−·316	28	·269
14	20	·606	30	·423	−·183	50	·481
15	1	·030	2	·028	−·002	3	·029
16	12	·364	20	·282	−·082	32	·308
17	22	·666	62	·873	+·207	84	·808
18	1	·030	6	·084	+·054	7	·067
19	17	·515	10	·141	−·374	27	·260
20	3	·091	5	·070	−·021	8	·077
21	..	..	4	·056	..	4	·038
22	10	·303	15	·211	−·092	25	·240
23	1	·030	3	·042	+·012	4	·038
24	..	..	5	·070	..	5	·045
25	..	..	9	·127	..	9	·085
26	..	..	1	·014	..	1	·010
27	..	..	2	·028	..	2	·019
28	..	..	1	·014	..	1	·010
29	..	..	6	·084	..	6	·058
30	..	..	8	·113	..	8	·077
31	..	..	1	·014	..	1	·010
32	..	..	6	·084	..	6	·058
33	..	..	1	·014	..	1	·010
34	..	..	5	·070	..	5	·048

On running the eye along the column of differences it will be seen that the number of spots in which an increase has taken place during the last six lunations is nearly equal to that in which a decrease has occurred, viz. ten of the former and eleven of the latter. Spot No. 3, a craterlet, has manifested the greatest increase, while spot No. 19 has exhibited the largest decrease.

The extent of variation of the separate spots is very irregular, and does not appear to indicate the operation of any general law. In one or two instances only have neighbouring spots been similarly affected; thus spots Nos. 5 and 14 in the S.W. quadrant of *Plato*, exhibit the same *decrease* of visibility, and the way in which they have varied from lunation to lunation is somewhat similar, and unlike the manner in which most of the other spots have varied. Spots No. 2 and 18 exhibit the same *increase* of visibility. The great increase of white spots in every part of the Moon's disk, about the time of full, dependent upon the value of ☾ — ☉ would, lunation after lunation, contribute to a steady value of the degree of visibility rather than the irregularity which is indicated by the observations if the *same* spots had been seen month after month. Although the observations amount to 771, and as many as twenty-two spots have been observed on one evening, the average number visible at any given time, as deduced from the 108 series of observations, is seven only, a number which is constant. Upon examining those series in which a smaller number than seven has been recorded, it is found that, besides the spots most commonly seen, viz. Nos. 1, 3, 4, and 17, the remaining two have not been the same. The additional spots seen on these occasions have been very various, several of them having low degrees of visibility, and some of these, which it might be expected could be seen only in the finest weather, have been observed in ordinary states of the Earth's atmosphere.

The observations of the twelve lunations ending in March, 1870, extend considerably the basis on which to found an intelligible explanation of the phenomena, it is, nevertheless, much too narrow to hazard more than conjecture. Another year's observations will doubtless throw further light on the subject.

On the Graphical Construction of the Umbral or Penumbral Curve at any instant during a Solar Eclipse. By Prof. Cayley.

The curve in question, say the penumbral curve, is the intersection of a sphere by a right cone,—I wish to show that the stereographic projection of this curve may be constructed as the envelope of a variable circle, having its centre on a given conic, and cutting at right angles a fixed circle; this fixed circle being in fact the projection of the circle which is the section of the sphere by the plane through the centre and the axis of the cone, or say by the axial plane. The construction thus arrived at is Mr. Casey's construction for a bicircular quartic; and it would not be difficult to show that the stereographic projection of the penumbral curve is in fact a bicircular quartic.

The construction depends on the remark that a right cone is the envelope of a variable sphere having its centre on a given *line, and its* radius proportional to the distance of the centre

from a given point on this line; and on the following theorem of plane geometry:

Imagine a fixed circle, and a variable circle having its centre on a given line, and its radius proportional to the distance of the centre from a given point on the line (or, what is the same thing, the variable circle always touches a given line); then the locus of the pole in regard to the fixed circle, of the common chord of the two circles (or, what is the same thing, the locus of the centre of a new variable circle which cuts the fixed circle at right angles in the points where it is met by the first-mentioned variable circle) is a conic.

To fix the ideas, say that P is the centre of the first variable circle; A B its common chord with the fixed circle; Q the centre of the circle which cuts the fixed circle at right angles in the points A and B; then the locus of Q is a conic.

To prove this, take $x^2 + y^2 = 1$ for the equation of the fixed circle $(x - \alpha)^2 + (y - \beta)^2 = \gamma^2$ for that of the variable circle; the foregoing law of variation being in fact such that α, β, γ, are linear functions of a variable parameter θ; the equation of the common chord A B is $- 2\alpha x - 2\beta y + 1 + \alpha^2 + \beta^2 - \gamma^2 = 0$; viz., this equation contains θ quadratically; hence the envelope of the common chord is a conic; and thence (reciprocating in regard to the fixed circle) the locus of the pole of A B, that is, of the point Q, is also a conic.

Consider now a solid figure in which the circles are replaced by spheres; viz. we have a fixed sphere, and a variable sphere having its centre on a given line and its radius proportional to the distance of the centre from a given point on the line. The envelope of the variable sphere is a right cone; the intersection of the cone with the fixed sphere is the envelope of the small circle of the sphere, say the circle A B, which is the intersection of the fixed sphere by the variable sphere. This circle A B is also the intersection of the fixed sphere by a sphere, centre Q, which cuts the fixed sphere at right angles; and by what precedes the locus of Q is a conic. Hence the penumbral curve is given as the envelope of the circle A B which is the intersection of the fixed sphere by a sphere which has its centre Q on a conic, and which cuts the fixed sphere at right angles. It is obvious that the circle A B always cuts at right angles the great circle which is the section of the fixed sphere by the axial plane, or say the axial circle. Project the whole figure stereographically; the projection of the circle A B is a variable circle which cuts at right angles the circle which is the projection of the axial circle, and which has for its centre the point Q′ which is the projection of Q. But the locus of Q being a conic, the locus of its projection Q′ is also a conic; and we have thus the projection of the penumbral curve as the envelope of a variable circle which has its centre on a conic, and which cuts at right angles a fixed circle.

We may in the axial plane construct five points of the conic which is the locus of Q, by means of any five assumed positions

of the variable circle, and somewhat simplify the construction by a proper choice of the five positions of the variable circle. This is not a convenient construction, and even if it were accomplished we should still have to construct the projection of the conic so obtained, in order to find, in the figure of the stereographic projection, the conic which is the locus of Q′. I do not at present perceive any direct construction for the last-mentioned conic; but assuming that a tolerably simple construction can be obtained, the construction of the projection of the penumbral curve as the envelope of the variable circle is as easy and rapid as possible. Probably the easiest course would be (without using the conic at all) to calculate numerically, for a given position of the variable sphere, the terrestrial latitude and longitude of the two points of intersection of the variable sphere by the axial circle; laying these down on the projection, we have then a position of the variable circle; and a small number of properly selected positions would give the penumbral curve with tolerable accuracy.

I have throughout spoken of the penumbral curve, as it is in regard hereto that a graphical construction is most needed; but the theory is applicable, without any alteration, to the umbral curve.

On the Geometrical Theory of Solar Eclipses. By Prof. Cayley.

The fundamental equation in a solar eclipse is, I think, most readily established as follows:—

Take the centre of the Earth for origin, and consider a set of axes fixed in the Earth and moveable with it; viz., the axis of z directed towards the North Pole; those of x, y, in the plane of the Equator; the axis of x directed towards the point longitude 0°; that of y towards the point longitude 90° W. of Greenwich. Take a, b, c, for the co-ordinates of the Moon; k for its radius (assuming it to be spherical); a', b', c', for the co-ordinates of the Sun; k' for its radius (assuming it to be spherical); then, writing $\theta + \phi = 1$, the equation

$$\{\theta(x-a)+\phi(x-a')\}^2+\{\theta(y-b)+\phi(y-b')\}^2+\{\theta(z-c)+\phi(z-c')\}^2 \\ =(\theta k \pm \phi k')^2$$

is the equation of the surface of the Sun or Moon, according as $\theta, \phi = 1, 0$ or $= 0, 1$: and for any values whatever of θ, ϕ, it is that of a variable sphere, such that the whole series of spheres have a common tangent cone. Writing the equation in the form

$$\begin{aligned}&\theta^2\{(x-a)^2+(y-b)^2+(z-c)^2-k^2\}\\ &+2\theta\phi\{(x-a)(x-a')+(y-b)(y-b')+(z-c)(z-c')\mp kk'\}\\ &+\quad\phi^2\{(x-a')^2+(y-b')^2+(z-c')^2-k'^2\}=0,\end{aligned}$$

or, putting for shortness,

$$\begin{aligned}
\varrho &= a^2 + b^2 + c^2 - k^2 \\
\varrho' &= a'^2 + b'^2 + c'^2 - k'^2 \\
\sigma &= a a' + b b' + c c' \mp k k' \\
\mathrm{P} &= a x + b y + c z \\
\mathrm{P}' &= a' x + b y + c' z,
\end{aligned}$$

the equation is

$$\begin{aligned}
&\theta^2\ (x^2 + y^2 + z^2 - 2\,\mathrm{P} + \varrho) \\
+\ &2\,\theta\varphi\ (x^2 + y^2 + z^2 - \mathrm{P} - \mathrm{P}' + \sigma) \\
+\ &\varphi^2\ (x^2 + y^2 + z^2 - 2\,\mathrm{P}' + \varrho') = 0
\end{aligned}$$

and the equation of the envelope consequently is

$$(x^2 + y^2 + z^2 - 2\,\mathrm{P} + \varrho)\,(x^2 + y^2 + z^2 - 2\,\mathrm{P}' + \varrho') - x^2 + y^2 + z^2 - \mathrm{P} - \mathrm{P}' + \sigma)^2 = 0$$

that is

$$(x^2 + y^2 + z^2)\,(\varrho + \varrho' - 2\,\sigma) - (\mathrm{P} - \mathrm{P}')^2 - 2\,(\varrho' - \sigma)\,\mathrm{P} - 2\,(\varrho - \sigma)\,\mathrm{P}' + \varrho\varrho' - \sigma^2 = 0$$

which is the equation of the cone in question.

Observe that one sphere of the series is a *point*, viz., taking first the upper signs, if we have $\theta\,k + \varphi\,k' = 0$, that is

$$\theta = \frac{k'}{k' - k},\quad \varphi = \frac{-k}{k' - k},$$

then the sphere in question is the point the co-ordinates whereof are

$$x = \frac{k'a - k\,a'}{k' - k},\quad y = \frac{k'b - k\,b'}{k' - k},\quad z = \frac{k'c - k\,c'}{k' - k}$$

which point is the vertex of the cone: it hence appears that, taking the upper signs, the cone is the *umbral* cone, having its vertex on this side of the Moon; and similarly taking the lower signs, then if we have $\theta\,k - \varphi\,k' = 0$, that is

$$\theta = \frac{k'}{k' + k},\quad \varphi = \frac{k}{k' + k},$$

then the variable sphere will be the point the co-ordinates of which are

$$\frac{k'a + k\,a'}{k' + k},\quad \frac{k'b + k\,b'}{k' + k},\quad \frac{k\,c' + k'c}{k' + k},$$

which point is the vertex of the cone; viz. the cone is here, the penumbral cone having its vertex between the Sun and Moon.

Taking as unity the Earth's equatorial radius, if p, p' are the

parallaxes, $\varkappa$, $\varkappa'$ the angular semi-diameters of the Moon and Sun respectively, then the distances are $\frac{1}{\sin p}$, $\frac{1}{\sin p'}$ and the radii are $\frac{\sin \varkappa}{\sin p}$, $\frac{\sin \varkappa'}{\sin p'}$ respectively; hence, if h, h' are the hour-angles west from Greenwich, Δ, Δ' the N.P.D.'s of the Moon and Sun respectively, we have

$$
\begin{aligned}
a &= \frac{1}{\sin p}\sin\Delta\cos h, \quad a' = \frac{1}{\sin p'}\sin\Delta'\cos h',\\
b &= \frac{1}{\sin p}\sin\Delta\sin h, \quad b' = \frac{1}{\sin p'}\sin\Delta'\sin h',\\
c &= \frac{1}{\sin p}\cos\Delta \qquad\quad, \ c' = \frac{1}{\sin p'}\cos\Delta',\\
k &= \frac{\sin \varkappa}{\sin p} \qquad\qquad\ , \ k' = \frac{\sin \varkappa'}{\sin p'}.
\end{aligned}
$$

And thence

$$
\begin{aligned}
\varrho &= \frac{1}{\sin^2 p}(1 - \sin^2 \varkappa'),\\
\varrho' &= \frac{1}{\sin^2 p'}(1 - \sin^2 \varkappa'),\\
\sigma &= \frac{1}{\sin p \sin p'}[\cos\Delta\cos\Delta' + \sin\Delta\sin\Delta'\cos(h'-h) \mp \sin\varkappa\sin\varkappa'],\\
\mathrm{P} &= \frac{1}{\sin p}\left\{\sin\Delta\,(x\cos h + y\sin h) + z\cos\Delta\right\},\\
\mathrm{P}' &= \frac{1}{\sin p'}\left\{\sin\Delta'(x\cos h' + y\sin h') + z\cos\Delta'\right\},
\end{aligned}
$$

Moreover, if the right ascensions of the Moon and Sun are α, α' respectively, and if the R.A. of the meridian of Greenwich (or sidereal time in angular measure) be $= \Sigma$, then we have

$$h = \Sigma - \alpha, \ h' = \Sigma - \alpha'.$$

It is to be observed that $h - h'$, Δ, Δ' are slowly varying quantities, viz., their variation depends upon the variation of the celestial positions of the Sun and Moon; but h and h' depend on the diurnal motion, thus varying about 15° per hour; to put in evidence the rate of variation of the several angles h, h', Δ, Δ' during the continuance of the eclipse, instead of the foregoing values of h, h', I write

$$h' = \left\{\mathrm{E} + \left(1 + \frac{\mathrm{E}_1 - \mathrm{E}}{24}\right)t\right\}15^\circ,$$

there t is the Greenwich mean time, E, E_1 are the values (reckoned in parts of an hour) of the Equation of Time at the preceding and following mean noons respectively, taken positively or negatively, so that E, E_1 are the mean times of the two successive apparent noons respectively; whence also

$$h = \left\{ E + \left(1 + \frac{E_1 - E}{24}\right) t \right\} 15^\circ - \alpha + \alpha'.$$

And moreover

$$\begin{aligned} \alpha &= A + m\,(t - T), \\ \alpha' &= A + m'\,(t - T), \\ \Delta &= D + n\,(t - T), \\ \Delta' &= D' + n'\,(t - T), \end{aligned}$$

if T be the time of conjunction, A, A, D, D′ the values at that instant of the R.A.'s and N.P.D.'s ; m, m' and n, n' the horary motions in R.A. and N.P.D. respectively.

It appears to me not impossible but that the foregoing form of equation

$$(x^2 + y^2 + z^2)(\varrho + \varrho' - 2\,\sigma) - (P - P')^2 - 2(\varrho' - \sigma)P - 2(\varrho - \sigma)P' + \varrho\varrho' - \sigma^2 = 0$$

for the umbral or penumbral cone might present some advantage in reference to the calculation of the phenomena of an eclipse over the Earth generally: but in order to obtain in the most simple manner the equation of the same cone referred to a set of principal axes, I proceed as follows :—

Writing

$$\begin{aligned} \mathrm{a} &= b\,c' - b'\,c, & \mathrm{f} &= a - a', \\ \mathrm{b} &= c\,a' - c'\,a, & \mathrm{g} &= b - b', \\ \mathrm{c} &= a\,b' - a'\,b, & \mathrm{h} &= c - c', \end{aligned}$$

(and therefore $\mathrm{af} + \mathrm{bg} + \mathrm{ch} = 0$)

Then, if

$$X = \frac{(\mathrm{bh} - \mathrm{cg})\,x + (\mathrm{cf} - \mathrm{ah})\,y + (\mathrm{ag} - \mathrm{bf})\,z}{\sqrt{\mathrm{a}^2 + \mathrm{b}^2 + \mathrm{c}^2}\,\sqrt{\mathrm{f}^2 + \mathrm{g}^2 + \mathrm{h}^2}},$$

$$Y = \frac{\mathrm{a}x + \mathrm{b}y + \mathrm{c}z}{\sqrt{\mathrm{a}^2 + \mathrm{b}^2 + \mathrm{c}^2}},$$

$$Z = \frac{\mathrm{f}x + \mathrm{g}y + \mathrm{h}z}{\sqrt{\mathrm{f}^2 + \mathrm{g}^2 + \mathrm{h}^2}},$$

X, Y, Z, will be co-ordinates referring to a new set of rectangular axes ; viz., the origin is, as before, at the centre of the Earth, the axis of Z is parallel to the line joining the centres of the Sun and Moon ; the axis of X cuts at right angles the last-mentioned line ; and the axis of Y is perpendicular to the plane

of the other two axes; or, what is the same thing, to the plane through the centres of the Earth, Sun, and Moon.

The co-ordinates of the vertex of the cone are therefore X_0, Y_0, Z_0, where these denote what the foregoing values of X, Y, Z, become on substituting therein for x, y, z, the values

$$\frac{k'a \mp k a'}{k' \mp k}, \qquad \frac{k'b \mp k b'}{k' \mp k'}, \qquad \frac{k'c \mp k c'}{k' \mp k},$$

and the equation of the cone therefore is

$$(X - X_0)^2 + (Y - Y_0)^2 = \tan^2 \lambda \, (Z - Z_0)^2,$$

where

$$\sin \lambda = \frac{k' \mp k}{G}$$

if for a moment G denotes the distance between the centres of the Sun and Moon. We have therefore

$$\tan \lambda = \frac{k' \mp k}{\sqrt{G^2 - (k' \mp k)^2}},$$

or since

$$G^2 = (a' - a)^2 + (b' - b)^2 + (c' - c)^2,$$

this is in fact

$$\tan \lambda = \frac{k' \mp k}{\sqrt{\varrho + \varrho' - 2\sigma}},$$

where ϱ, ϱ', σ signify as before; and thus X_0, Y_0, Z_0, $\tan \lambda$ are all of them given functions of $a, b, c, k, a', b', c', k'$, and consequently of the before-mentioned astronomical data of the problem. The form is substantially the same as Bessel's equation (3), *Ast. Nach.* No. 321 (1837), (but the direction of the axes of X, Y, is not identical with those of his x, y); and it is therefore unnecessary to consider here the application of it to the calculation of the eclipse for a given point on the Earth.

The Astronomische Gesellschaft.

The third biennial meeting was held at Vienna from the 13th to the 16th of September, 1869, under the presidency of M. Struve; the number of members present was 39; total number 216. The several subjects in question at the meeting of 1867 (*See Monthly Notices*, vol. xxviii. p. 268) were further discussed, and other subjects brought before the meeting; the principal ones were as follows:—

1. As to the New Tables of *Jupiter*, a large part of the

auxiliary calculations had been performed under the direction of Prof. Hansen, by junior members of the Society; and (his own part of the work being already accomplished) it was hoped that in the following year the definitive amendment of the elements could be taken in hand.

2. The reduction of the observations of the Periodic Comets had been proceeded with; a desideratum in reference thereto had been supplied by the completion of the *Tabulæ Quantitatum Besselianarum pro Annis* 1750 *ad* 1840, under the direction of MM. Struve and Auwers.

3. In reference to the new reduction of Bradley's observations, Prof. Auwers reported that the more mechanical part of the work was already accomplished. The number of zenith distances observed between 1750 and 1762 (exclusively of those of the Moon and planets, which were not in the first instance to be included in the new reduction) was about 19,000, of which 1650 related to fundamental stars.

4. Dr. Schmidt, Director of the Observatory at Athens, exhibited eight sheets of his new map of the Moon; the plan being that of Lohrman's, but the scale about double, the diameter being 6 Paris feet. It was stated that the publication of the map would hardly be completed within 10 years.

5. Plans were exhibited of the proposed New Observatory at Vienna.

6. In reference to the plan for the observation of the stars of the northern hemisphere up to the ninth magnitude, reports were received from MM. Struve, Kowalski, Krüger, Schwarz, Bruhns, Tiele, Auwers, and Safford, upon the progress which had been made: and a definitive programme was drawn up, which is published in the *Vierteljahrschrift.* It appears thereby that the work has been undertaken by the different observatories in zones as follows:—

80°	to	75°	Kasan.
75	,,	70	Dorpat.
70	,,	65	Christiania.
65	,,	55	Helsingfors.
55	,,	50	*Vacant.*
50	,,	40	Bonn.
40	,,	35	Chicago.
35°	,,	30°	Leipzig.
30	,,	25	Cambridge (E.)
25	,,	15	Berlin.
15	,,	10	Leipzig.
10	,,	4	Mannheim.
4	,,	1	Neuchatel.
+ 1	,,	− 2	Palermo.

7. Prof. Förster presented a report on the Eclipse-expedition

of 1868, which was intended to be published as a supplement to the *Vierteljahrschrift.*

8. Some discussion took place as to the proposed expedition for the observation of the transit of 1874.

The next meeting was fixed to be at Stuttgard, in 1871, under the presidency of M. Struve.

Besides the *Vierteljahrschrift* and the several works referred to *Monthly Notices*, vol. xxviii., there have been published by the Society.

Supplement-heft zu Jahrgang III.. von Asten Dr. E. *Neue Hulfstafeln zur Reduction der in der Histoire Céleste Française enthaltenen Beobachtungen*, 1868.

9. Lesser Dr. Otto. *Tafeln der Pomona mit Berücksichtigung der Störungen durch Jupiter, Saturn und Mars berechnet*, 1869.

Instrument for Sale.

A large spectroscope, recently constructed by Messrs. Simms for Mr. Huggins. The prisms are by Hofmann. It is adapted for use with the telescope, and suitable for observation of nebulæ, stars, and the Sun. It can be used for table purposes. To be seen at Mr. Ladd's, Beak Street, Regent Street, W.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *May* 13, 1870. No. 7.

WILLIAM LASSELL, Esq., President, in the Chair.

Edward Dent, Esq., 12 Hyde Park Gardens;
James Dyson Perrins, Esq., Davenham Bank, Malvern; and
Charles Henry Gatty, Esq., Felbridge Park, East Grinsted,
were balloted for and duly elected Fellows of the Society.

Meteorological Observations taken at Gibraltar. By Lieut. Alexander B. Brown, R. A.

Since some discussion has been entered into with respect to a suitable station for observing the total phase of the Solar Eclipse of December 22nd, 1870; and as it is absolutely necessary for the satisfactory carrying out of our experiments that we should have a clear or moderately clear sky, I have much pleasure in submitting to the Society some extracts from my meteorological notes taken in Gibraltar from 1860 to 1867. From that date (1867) to the present time I have fallen back (for the last two years) to well authenticated observations.

I should remark that at Gibraltar,—

b means blue sky, quite or almost quite free from cloud;
c means much or little blue sky with cloud;
f, fog; *m*, mist; *g*, gloomy; *r*, rain; *p*, passing showers.

As respects force of wind,—

1, light air;	2, light breeze;
3, gentle breeze;	4, moderate breeze;
5, fresh breeze;	12, hurricane.

Notes on 22nd December.

	Wind.	Force.	Barometer.	Sky.
1860, Dec. 22	SW, W	6	29·93	Fog
1861, ,,	SW, SSW	5	29·78	Showery
1862, ,,	WNW	3	30·01	Blue sky
1863, ,,	NE, WNW	2	30·23	Blue sky
1864, ,,	ENE, NE	2	30·00	Blue sky, with cloud
1865, ,,	SE, ESE	4	30·25	Cloudy
1866, ,,	E	3	30·24	Blue sky, with cloud
1867, ,,	W	4	30·16	Blue sky
1868, ,,	WSW, SW	5	30·13	Blue sky
1869, ,,	WNW	5	30·07	Blue sky, with cloud

Meteorological Notes of 17 Days, from 15 to 31 December (inclusive) for past 10 years.

17 Days. 15–31 December.	Average Wind.	Force of Wind.	*b.*	*c.*	*f, m.*	*g.*	*r.*	*p.*
1860	WNW, SW	5	6	6	4	1	..	..
1861	NE, SE	4	3	5	1	1	6	1
1862	WNW, ENE	3	10	6	1	..	..	..
1863	WNW	3	13	3	..	..	1	..
1864	WNW	4	5	10	1	..	..	1
1865	E, ENE, ESE	2	12	4½	..	..	½	..
1866	E, ESE	4	6	9	2	..	..	..
1867	W, WNW	3	8	5	1	..	½	2½
1868	WNW	5	11	5	..	..	1	..
1869	WNW	5	7	9	½	..	..	½

The numbers in the last six columns show for each year how many of the 17 days were *b*, *c*; *f*, *m*; *g*, *r*, or *p*, respectively; the total in each line being of course = 17. The observations being taken at 9 A.M. and 3 P.M., ½ days have been introduced.

We thus see then, out of the last 10 returns of 22nd December, there were

4 very good observing days;
3 fair ditto;
1 not quite so good;
1 in which probably little could be done; and
1 useless for observation of the Sun.

That from the 15th to 31st December in the

Years 1860	6 very good days	6 fair days	5 indifferent
1861	3 ,,	5 ,,	9 ,,
1862	10 ,,	6 ,,	1 ,,
1863	13 ,,	3 ,,	1 ,,

Years	1864	5 very good days	10 fair days	2 indifferent
	1865	12 „	$4\frac{1}{2}$ „	$\frac{1}{2}$ „
	1866	6 „	9 „	2 „
	1867	8 „	5 „	3 „
	1868	11 „	5 „	1 „
	1869	7 „	9 „	1 „

which I think we may consider very satisfactory.

On his Photographs taken during the Total Solar Eclipse, Aug. 7, 1869. By Commander Ashe.

(*Letter addressed to the Editor of the Monthly Notices.*)

In the *Monthly Notices* of the Astronomical Society of Feb. 11, 1870, in the Council Report, at page 108 (in reference to the four negatives taken during totality at Jefferson Town) are these words,—"Unfortunately, in photographs 3 and 4, there is evidence of the disturbance of the telescope during the exposure of the sensitive plate."

Now I beg to state there is no evidence of the sort, and by the enclosed print of an enlarged copy of No. iv. I clearly prove that no disturbance of the telescope took place. I have given ample evidence to the Astronomer Royal that the telescope was not disturbed; but if there was nothing else, the photograms taken at Des Moines (the next station to us), would prove it, for, in the photogram taken near the end of totality, are seen the stumps of the three parallel planes of light that are seen in No. iv. photogram at E. My negative was taken as near the limb of the Sun as it is possible to take it, for just as I closed the slide the Sun burst out, and this remarkable photogram shows two bright bands divided by a dark band. What are they? They are crossed by rays of light that do not radiate from the Sun's centre, but are all parallel to each other. It must be remembered that no other party was looking at the totality at the same time that we were, for just as it ended at Jefferson it commenced at Des Moines.

Now, with reference to my photograms "confirming what has already been stated in regard to the identity of form preserved by the protuberances," I will now relate a fact that was seen by all at Jefferson, and by Mr. Falconer, whose address is Alexander Plytts Falconer, Esq., Bath County Club, Bath, and may be referred to. The point of light that is seen in No. i., and is described by all the American astronomers, on the "great protuberance"—whilst I was waiting for No. iii. plate—shot out to an enormous length, at least one-third longer than it is seen in No. iii. photogram; and when it reached its greatest height (which

it did in a few seconds), the top was blown off at right angles and came to a point, just like a flame acted on by a blow-pipe. The lower part was deep red, getting lighter, and the part blown off was a brilliant white light. Now then let us investigate the evidence. Although this is my second total eclipse, still my surprise and love of the marvellous might be supposed to have produced in my bewildered mind an impression that did not exist in reality. If I were the only one who saw it, then it might very readily be put down to fancy, but all those who came out from the town, and were standing round the observatory, saw it, and the crowd produced that sensational murmur that they did when the totality commenced. One man described it as a crooked piece of iron taken from a forge "white hot" at the top. But Mr. Falconer, who was then travelling in America, gave me a very good drawing of it, and who no doubt will, if written to, corroborate my statement.

Now I state that a few seconds after it had reached its greatest height, it lost the flame-like appearance, and became a duller red, and quickly reduced its height; and when No. iii. negative was taken, it was apparently a cinder; and when this negative is examined by a lens, it shows cracks and various lines, that were also seen by Mr. Vail, who observed the eclipse with a small telescope. Now, supposing that we *all* agreed to foist this marvellous story on the public, still there is the photogram No. iii., which shows an enormous protuberance, which was fast crumbling away; and on examining No. iv. negative, we see it greatly reduced; but still it has the same characteristic form. Now look at the "Des Moines" photogram. Here we see that it has assumed the form of a great heap of cinders, but the long exposure, 66 seconds, has softened the outlines.

After the protuberance has taken this form, then it naturally retains it for some time, and thus all the other stations to the eastward have very similar photograms.

In common justice to our party I have to request that this communication may be printed in the *Monthly Notices* of the Society. And, in conclusion, I beg that the negatives may be returned to me, as I am about to print my Report.

Observatory, Quebec, April 14, 1870.

A Committee appointed by the Council unanimously report that, in their opinion, there was a decided movement of the instrument at the time the photograph was taken. This conclusion they arrived at from an examination of the chromosphere close to the Moon's limb, as well as from an examination of the prominences.

Observations of an Occultation of Saturn by the Moon; of Occultations of Stars by the Moon; and of Phenomena of Jupiter's Satellites; made at the Royal Observatory, Greenwich, from 1869, *April, to* 1870, *April.*

(*Communicated by the Astronomer Royal.*)

Occultation of Saturn (Disappearance and Reappearance), 1870, *April* 19.

Phenomenon.	Greenwich Mean Solar Time of Observation. Hour and Minute.	Seconds observed by E.	C.	J. C.
	h m	s	s	s
First Contact with Ring	14 55	49·8	55·5	50·8
First Contact with Ball	14 56	13·8	17·4	16·7
Total Disappearance of Ball	14 56	44·7	53·3	60·1
Total Disappearance of Ring	14 57	15·6	16·8	24·6
First Reappearance of Ring	16 4	...	34·2	33·5
First Reappearance of Ball	16 5	...	0·1	0·4
Last Contact with Ball	16 5	...	40·5	44·3
Last Contact with Ring	16 6	...	5·4	6·2

Notes by E.—With the Altazimuth and a power of about 100. The first three times probably uncertain to 1^s or 2^s; the last still more uncertain, the light of the planet by contrast with the Moon being extremely faint.

By C.—With the Sheepshanks Equatoreal and a power of 64. The observations of the disappearance were not considered good, owing to the great faintness of the planet on approaching the bright limb of the Moon; the first contact with the limb of *Saturn* being the best. The observations of the reappearance were considered pretty good; those of the first limb of the planet and last contact of ring being thought the most accurate. There was not the least trace of alteration of the planet's form noticed; in fact, the Moon's limb at the reappearance seemed to divide the planet with extraordinary sharpness. Owing to the amount of colour on *Saturn*, in order to get a good image I reduced the aperture of the object-glass to 4½ inches; this gave a very nice image indeed.

By J. C.—With the Great Equatoreal and a power of 340. At disappearance the planet was a very dull object when in contact with the Moon; its light probably a twentieth as bright. The times noted are probably correct to a second, except the last, which is doubtful to two or three seconds. At reappearance the planet was rather tremulous; no distortion whatever was noticed.

Occultations of Stars by the Moon.

Day of Observation.	Phenomenon.	Moon's Limb.	Mean Solar Time.	Observer.
1869.			h m s	
June 23	(*a*) μ^1 Sagittarii, disappearance	Bright	10 53 22·5	D.
Aug. 2	Aldebaran, reappearance	Dark	13 13 39·8	J. C.
Dec. 14	ξ^2 Ceti, disappearance	Dark	9 20 17·5	E.
1870.				
Feb. 10	*m* Tauri, disappearance	Dark	9 8 16·3	E.

(*a*) The star very faint at disappearance.

Phenomena of Jupiter's Satellites.

Day of Obs.	Satellite.	Phenomena.	Mean Solar Time. h m s	Observer.
1869. Oct. 5	I.	Eclipse, disappearance	12 44 55·9	H.C.
21	III.	Eclipse, disappearance	8 50 45·6	C.
	III.	Eclipse, reappearance	10 32 43·9	C.
	III.	(*a*) Occult. disappearance, bisection	10 55 10·2	C.
	I.	Eclipse, disappearance	11 2 59·0	C.
Nov. 15	III.	Transit, ingress, first contact	10 13 16·0	J. C.
	III.	" " bisection	10 21 44·6	J. C.
	III.	Transit, egress, bisection	11 51 29·8	J. C.
	III.	" " last contact	11 57 58·7	J. C.
19	II.	Eclipse, reappearance	13 17 37·2	J. C.
Dec. 14	II.	Eclipse, reappearance	10 22 31·7	E.
15	I.	Eclipse, reappearance	10 3 21·1	D.
1870. Jan. 30	I.	(*b*) Eclipse, reappearance	10 37 24·5	S.
Feb. 27	III.	Eclipse, disappearance	9 23 30·0	S.
Mar. 13	II.	(*c*) Eclipse, reappearance	6 52 17·7	J. C.
Apr. 4	III.	(*d*) Eclipse, reappearance	7 18 26·3	E.

(*a*) Owing to the path of the satellite cutting the disc of the planet at a very small chord, and the occultation being little more than a graze, it was impossible to estimate with any accuracy the time of occultation.

(*b*) A haze prevalent; the time noted probably somewhat late.

(*c*) The sky rather bright from daylight.

(*d*) Very faint; the time noted is that at which the satellite was first seen; it could not have been visible more that a few seconds previously.

The initials S., D., E., C., J. C., and H. C. are those of Mr. Stone, Mr. Dunkin, Mr. Ellis, Mr. Criswick, Mr. Carpenter, and Mr. H. Carpenter.

Occultation of Saturn by the Moon, Tuesday, April 19, 1870. By Capt. W. Noble.

As this was my first view of *Saturn* this year I occupied myself, from $14^h\ 40^m$ L.M.T., in scrutinising the physical features of the planet before the occultation. I employed a power of 255 on my 4·2-inch Equatoreal, the same with which I subsequently observed the occultation itself.

Notwithstanding *Saturn's* small altitude he was well and sharply defined, Ball's division being visible over the North Pole. The shadow of the ball was of course to the west of it on the rings. The crape ring C was seen in the ansæ very distinctly. *Saturn* appeared of a richly greenish yellow when compared with the brilliant white light of the Moon.

The Occultation.

The first contact of the outer edge of ring A with the Moon's bright limb took place at $16^h\,47^m\,55^s$ L.S.T. $= 14^h 55^m\,55^s\!\cdot\!6$ L.M.T. and that of the inner edge of ring B at $16^h\,48^m\,8^s\!\cdot\!6$ L.S.T. $=$ $14^h\,56^m\,9^s\!\cdot\!2$ L.M.T. The preceding limb of the planet touched that of the Moon at $16^h\,48^m\,19^s$ L.S.T. $= 14^h\,56^m\,19^s\!\cdot\!6$ L.M.T. The globe of *Saturn* was dichotomised (as nearly as I could estimate) at $16^h\,48^m\,33^s$ L.S.T. $= 14^h\,56^m\,33^s\!\cdot\!5$ L.M.T. His following limb disappeared at $16^h\,49^m\,1^s$ L.S.T. $= 14^h\,57^m\,1^s\!\cdot\!4$ L.M.T. The inner edge of ring B was occulted at $16^h\,49^m\,13^s$ L.S.T. $= 14^h\,57^m\,13^s\!\cdot\!4$ L.M.T., and the last perceptible trace of the ring vanished at $16^h\,49^m\,25^s$ L.S.T. $= 14^h\,57^m\,26^s\!\cdot\!4$ L.M.T. Although very pale the planet was perfectly distinct, and passed behind the Moon's limb without wave, shake, or distortion.

At the reappearance of *Saturn* the first visible trace of the edge of the preceding ansa was caught sight of about $17^h\,57^m\,19^s$ L.S.T. $= 16^h 5^m\,8^s\!\cdot\!3$ L.M.T., and at $17^h\,58^m\,1^s$ L.S.T. $= 16^h\,5^m\,50^s\!\cdot\!1$ L.M.T. the planet was *just* clear of the Moon's limb.

The emersion was very striking, from the exceeding sharpness of *Saturn;* the most delicate detail being perceptible, even in contact with the lunar limb. The crape ring C was seen most perfectly where the dark limb of the Moon crossed it. I never was more impressed with the absolute absence of a lunar atmosphere of any appreciable density than I was on this occasion.

Forest Lodge, Maresfield, Sussex,
May 13, 1870.

Occultation of Saturn. By C. G. Talmage, Esq.

The occultation of *Saturn* was well seen here: at sunset the sky was quite clear, and remained so to sunrise. *Saturn* was visible to the naked eye to within three minutes and a-half of the time of disappearance.

When I first looked at *Saturn*, at about 13^h, no striking difference of colour from the Moon was visible, but by 14^h the difference was quite perceptible, and at $14^h\,45^m$ it was most marked, the planet appearing of a yellow tint.

I had no difficulty whatever in observing both the disappearance and reappearance of *Titan*. To prevent the glare of the Moon I covered the eyepiece half over with silver foil, so that the eye was greatly relieved. The field was, therefore, of the following shape:—

The local mean times are as follows:—

	h m s
Disappearance of *Titan*	= 14 49 18·97
First contact with ring	14 55 51·90
,, ,, ball	14 56 15·84
Final disappearance of ball	14 56 57·73
,, ,, ring	14 57 17·68
Reappearance of *Titan*	15 58 56·53
,, ,, ring	16 4 31·63
,, ,, ball	16 4 58·56
Ring clear of Moon	= 16 6 1·89

I used a power of 110 on the 10-inch refractor.

I believe, had I taken the precaution of getting out the positions of the faint satellites, I could have observed their occultations, as I saw several very faint objects occulted, both preceding and following *Saturn*, and the field was full of stars.

The position of the Observatory is—

Lat. 51° 34′ 34″ N.
Long. $0^h\ 0^m\ 0^s{\cdot}87$ W.

Mr. Barclay's Observatory, Leyton.

Occultation of Saturn. By John Joynson, Esq.

The occultation of *Saturn* by the Moon, on the 19th April last, was seen here as satisfactorily as could be expected, considering the low altitude of the planet. The sky was quite free from cloud at the disappearance, but at the reappearance there was just sufficient hazy cloud about the Moon to interfere with the correct observation of the exact time of the egress of the preceding edge of the ring.

The following were the times noted:—

Disappearance.

			h m s	
First contact of ring			14 52 52·7	G.M.T.
Inner	ditto	ditto	53 2·2	
First	ditto	of ball	53 22·1	
Last	ditto	ditto	53 39·6	
Inner	ditto	of ring	54 2·0	
Last	ditto	ditto	14 54 10·1	

Reappearance.

First appearance of ring.		Not noted, for hazy cloud.	
Inner edge	ditto	Not sure	ditto

	h m s	
First limb of ball	16 0 29·1	G.M.T.
Last ditto ditto	0 46·0	
Inner edge of ring	1 7·3	
Last ditto ditto	16 1 15·9	

Lens 3½ inches; Power 110.

Lat. 53° 28′ 24″ N.
Long. 0^h 12^m $6^s.9$ W.

Waterloo, near Liverpool,
May 10, 1870.

Observations of Algol; of Occultations of Stars and of Saturn by the Moon; and of Sun-spots. By C. F. Penrose, Esq.

The position is

Latitude 51° 24′ 58 N.
Longitude 0^h 0^m 55·1 W.

Algol.

			G.M.T.
Minima observed by estimation	1869	Oct. 11	10^h 50^m
	and	Dec. 18	$6^h.7$

These observations, which represent the phenomena certainly within 10 minutes, show that the period of 2·86727 days which has been assigned, combined with an epoch given for Jan. 3, 1844, in the *Outlines of Astronomy*, requires a slight correction. These minima occurred nearly 3 hours earlier than if due to those data. The value 2·867234 would better satisfy them.

Occultations as follows:—

1869, Aug. 2.

			G.M.T.
Aldebaran	Reapp.	Dark limb	13^h 13^m 37^s

1870, Feb. 2.

m Tauri	Disapp.	Dark limb	9^h 7^m $14^s.9$

April 19th.

Saturn.

At Disapp. Bright limb

		G.M.T. h m s
First contact of Ring	..	14 55 30
Ball apparently bisected	..	56 17
Final disappearance of Ring	..	57 5

At Reapp.	Dark limb	
		h m s
Ball bisected ..	..	16 5 2
Ring clear of Moon	..	5 52

Of these occultations the two first have been subjected to calculation. That of *Aldebaran* accords with the longitude within a very few seconds. In the case of *m Tauri* the discordance is greater (viz. about 13^s), but the occultation was far from a central one. The Moon seems to have been behind, or above, or both, as repects her tabular place.

Several Sun-spots have been noticed exhibiting a remarkable appearance when near the limb, especially on March 25 and April 25, a sketch of the latter is submitted.

If the Sun-spots are cuplike or conical depressions and symmetrically placed, or nearly so, with respect to a normal or solar plumbline, the breadth of the nearer margin would invariably be less than that of the further margin when near the limb, and would even disappear on approaching it (which is the general phenomenon). That, of which the sketch is submitted, on April 25, was about 25° from the limb, and exhibited its nearer margin equal in breadth to the further one. By the imaginary section through the photosphere it is shown how very oblique must have been the axis of the cavity around the spot on the hypothesis of its cuplike shape.

Colebyfield, Wimbledon.

Note respecting η Argûs. By H. A. Severn, Esq.

(*Extract of Letter addressed to the Astronomer Royal by Henry A. Severn, Union Bank of Australia, Melbourne, Victoria, received April* 1870.)

"I may say that I cannot confirm the new position given to *η Argûs* in respect to the Nebula. I have watched it for 14 years, and it is just where it was; of course much less brilliant."

Instruments, 13 in. front view reflector, of his own construction, and a $3\frac{3}{4}$ in. refractor.

Comparisons of the Places of certain Stars, as given in the Second Radcliffe Catalogue, with the Places given by Dr. Wolfers in the Tabulæ Reductionum. By Dr. Wolfers. Translation.

Extract of a Letter from Dr. Wolfers to the Radcliffe Observer.

"I beg to offer my best thanks for the copy which I have received of the Second Radcliffe Catalogue of Stars, presented to me by the Radcliffe Trustees.

"You will not be surprised that I have, as on former occasions, compared the mean places of the stars therein contained with the places determined by me, since the latter are now in use at several observatories. The agreement is, as in all former comparisons, in general very satisfactory, as you will see by the accompanying copy. I must leave it to your own judgment to determine whether it is proper to communicate the paper to the Royal Astronomical Society. It will be a satisfaction to me if, by this means, the use of the *Tabulæ Reductionum* should be extended.

Comparison of Fundamental Stars (Tab. Red. = W.) *with the Second Radcliffe Catalogue* (M.)

Name.	No. of Obs.	R.A. M.	R.A. W.	R.A. M.—W.	No. of Obs.	Declination. M.	Declination. W.	Declination. M.—W.
		h m s	s			° ′ ″	″	″
α Andromedæ	12	0 1 9.38	9.43	−0.05	7	+28 19 3.7	3.3	+0.4
γ Pegasi	21	0 6 1.77	1.86	−0.09	6	+14 24 21.0	18.5	+2.5
α Cassiopeiæ	3	0 32 35.14	35.06	+0.08	6	+55 46 7.3	8.4	−1.1
α Arietis	14	1 59 17.25	17.32	−0.07	2	+22 47 56.4	55.2	+1.2
α Ceti	18	2 54 57.89	57.87	+0.02	4	+ 3 32 17.4	17.0	+0.4
α Persei	5	3 14 20.74	20.80	−0.06	4	+49 21 33.0	33.2	−0.2
α Tauri	19	4 27 53.43	53.46	−0.03	11	+16 13 28.4	28.4	0.0
α Aurigæ	10	5 6 21.06	21.17	−0.11	9	+45 51 2.8	3.4	−0.6
β Orionis	11	5 7 48.62	48.70	−0.08	9	− 8 21 59.2	59.5	+0.3
β Tauri	7	5 17 26.69	26.67	+0.02	7	+28 29 8.2	5.9	+2.3
α Orionis	13	5 47 35.58	35.64	−0.06	11	+ 7 22 38.8	38.2	+0.6
α Canis Maj.	10	6 38 58.63	58.70	−0.07	24	−16 31 36.6	36.2	−0.4
α Geminorum	13	7 25 39.52	39.45	+0.07	15	+32 11 30.5	29.4	+1.1
α Canis Min.	15	7 31 58.35	58.36	−0.01	12	+ 5 34 50.8	50.5	+0.3
β Geminorum	17	7 36 44.64	44.69	−0.05	11	+28 21 39.7	38.9	+0.8
α Hydræ	15	9 20 42.43	42.47	−0.04	8	− 8 3 15.0	12.9	−2.1
α Leonis	20	10 0 54.71	54.78	−0.07	16	+12 38 60.6	59.9	+0.7
α Ursæ Maj.	5	10 55 3.51	3.28	+0.23	16	+62 30 21.4	20.7	+0.7
β Leonis	15	11 41 54.98	55.00	−0.02	6	+15 21 16.1	16.8	−0.7
β Virginis	11	11 43 24.20	24.16	+0.04	5	+ 2 33 12.3	12.9	−0.6
γ Ursæ Maj.	6	11 46 27.25	27.08	+0.17	4	+54 28 22.8	23.2	−0.4
α Virginis	21	13 17 49.29	49.32	−0.03	18	−10 25 46.8	45.3	−1.5
η Ursæ Maj.	13	13 42 1.27	1.25	+0.02	7	+50 0 46.6	47.8	−1.2
α Boötis	11	14 9 16.55	16.62	−0.07	5	+19 54 47.2	47.5	−0.3
δ Libræ	1	14 42 56.89	56.92	−0.03	1	−15 24 47.1	44.9	−2.2
α Libræ	14	14 43 8.32	8.36	−0.04	3	−15 27 27.8	26.5	−1.3
β Ursæ Min.	1	14 51 8.89	9.23	−0.34	7	+74 43 38.8	38.3	+0.5
α Coronæ	14	15 28 45.67	45.71	−0.04	5	+27 11 17.5	18.2	−0.7
α Serpentis	15	15 37 22.43	22.48	−0.05	4	+ 6 52 7.0	8.1	−1.1
α Scorpii	18	16 20 49.70	49.73	−0.03	10	−26 7 2.9	2.4	−0.5
α Herculis	11	17 8 15.87	15.95	−0.08	2	+14 33 11.0	11.0	0.0
α *Ophiuchi*	26	17 28 26.13	26.23	−0.10	11	+12 39 54.6	54.8	−0.2

Name.	No. of Obs.	R.A. M.	R.A. W.	R.A. M.—W.	No. of Obs.	Declination. M.	Declination. W.	Declination. M.—W.
		h m s	s			° ′ ″	″	″
γ Draconis	20	17 53 21·35	21·47	−0·12	6	+51 30 24·8	24·5	+0·3
α Lyræ	14	18 32 11·89	11·94	−0·05	14	+38 39 20·4	20·2	+0·2
γ Aquilæ	21	19 39 36·13	36·25	−0·12	9	+10 16 29·3	29·8	−0·5
α ,,	26	19 43 57·08	57·18	−0·10	15	+ 8 30 5·5	5·5	0·0
β ,,	20	19 48 26·19	26·21	−0·02	8	+ 6 3 34·9	35·6	−0·7
α¹ Capricorni	1	20 9 53·16	53·17	−0·01	4	−12 56 18·7	15·9	−2·8
α ,,	14	20 10 17·08	17·09	−0·01	7	−12 58 35·2	32·6	−2·6
α Cygni	26	20 36 39·47	39·59	−0·12	12	+44 46 52·7	54·2	−1·5
α Cephei	17	21 15 14·18	14·14	+0·04	20	+61 59 35·2	35·0	+0·2
α Aquarii	25	21 58 35·49	35·57	−0·08	6	− 0 59 55·1	54·0	−1·1
α Piscis Austr.	10	22 49 54·40	54·44	−0·04	11	−30 21 47·2	47·5	+0·3
α Pegasi	22	22 57 47·33	47·39	−0·06	9	+14 27 9·3	10·9	−1·6
α Ursæ Min.	14	1 8 2·37	2·65	−0·28	17	+88 33 46·9	47·1	−0·2
δ Ursæ Min.	4	18 17 30·60	30·16	+0·44	17	+86 36 7·4	6·2	+1·2

Comparison of Additional Stars (*Astr. Jahrbuch*, 1867, page 325, = (W)) *with the Second Radcliffe Catalogue* (M).

Name.	No. of Obs.	R.A. M.	R.A. W.	R.A. M.—W.	No. of Obs.	Declination. M.	Declination. W.	Declination. M.—W.
		h m s	s	s		° ′ ″	″	″
β Ceti	13	0 36 33·63	33·63	0·00	5	−18 45 21·9	21·3	−0·6
γ ,,	18	2 36 3·01	3·01	0·00	7	+ 2 38 37·8	37·1	+0·7
δ Arietis	17	3 3 37·74	37·82	−0·08	8	+19 11 40·3	40·1	+0·2
ι Ursæ Maj.	10	8 49 36·36	36·32	+0·04	9	+48 35 17·7	17·6	+0·1
θ ,,	4	9 23 28·32	28·28	+0·04	1	+52 18 48·4	46·3	+2·1
γ¹ Leonis	9	10 12 14·91	14·95	−0·04	4	+20 32 54·0	53·7	+0·3
χ ,,	13	10 57 47·60	47·70	−0·10	9	+ 8 5 30·7	30·9	−0·2
δ ,,	10	11 6 39·39	39·58	−0·19	8	+21 17 25·8	25·0	+0·8
δ Crateris	9	11 12 20·59	20·67	−0·08	7	−14 1 18·1	17·2	−0·9
γ Virginis med.	9	12 34 34·11	34·13	−0·02	4	− 0 40 53·3	51·0	−2·3
12² Canum Ven.	7	12 49 28·47	28·46	+0·01	3	+39 4 31·2	31·1	+0·1
ζ Virginis	11	13 27 33·58	33·82	−0·24	5	+ 0 7 14·9	17·9	−3·0
η Boötis	17	13 48 1·15	1·22	−0·07	6	+19 6 3·5	4·0	−0·5
ψ ,,	7	14 58 26·80	26·86	−0·06	4	+27 29 43·9	45·2	−1·3
ζ Ursæ Min.	2	15 49 8·72	8·53	+0·19	4	+78 13 27·5	24·0	+3·5
ζ Herculis	16	16 36 0·57	0·72	−0·15	7	+31 51 32·2	30·7	+1·5
κ Ophiuchi	17	16 51 2·53	2·56	−0·03	7	+ 9 35 44·0	45·3	−1·3
β Draconis	12	17 27 16·12	16·27	−0·15	15	+52 24 23·8	23·4	+0·4
μ Herculis	22	17 40 58·78	58·78	0·00	10	+27 48 18·9	19·9	−1·0
β¹ Lyræ	26	18 44 54·61	54·61	0·00	4	+33 12 9·6	9·0	+0·6
1 Aquilæ	27	19 18 26·30	26·34	−0·04	8	+ 2 50 19·8	19·9	−0·1
61¹ Cygni	18	21 0 37·43	37·39	+0·04	12	+38 3 47·8	47·3	+0·5
γ Piscium	22	23 9 54·42	54·47	−0·05	6	+ 2 31 3·4	4·8	−1·4
ι ,,	18	23 32 45·00	45·13	−0·13	13	+ 4 52 3·7	3·9	−0·2
ω ,,	11	23 52 7·34	7·46	−0·12	6	+ 6 5 16·1	17·9	−1·8

Note on Dr. Wolfers' Paper. Comparison between the Places of the Greenwich Seven-year Catalogue and Radcliffe Catalogue, 1860, for the Stars in Table I. By E. J. Stone, Esq.

	Greenwich Mean Right Ascension.			Rad.	G.—R.	Mean N.P.D.			Rad.	G.—R.
	h	m	s	s	s	°	′	″	″	″
α Andromedæ	0	1	9·44	9·38	+ 0·06	61	40	57·79	56·3	+ 1·5
γ Pegasi	0	6	1·79	1·77	+ 0·02	75	35	42·37	39·0	+ 3·4
α Cassiopeiæ	0	32	35·06	35·14	− 0·08	34	13	51·94	52·7	− 0·8
α Arietis	1	59	17·28	17·25	+ 0·03	67	12	5·60	3·6	+ 2·0
α Ceti	2	54	57·83	57·89	− 0·06	86	27	42·81	42·6	+ 0·2
α Persei	3	14	20·72	20·74	− 0·02	40	38	26·72	27·0	− 0·3
α Tauri	4	27	53·43	53·43	0·00	73	46	32·97	31·6	+ 1·4
α Aurigæ	5	6	21·12	21·06	+ 0·06	44	8	57·40	57·2	+ 0·2
β Orionis	5	7	48·63	48·62	+ 0·01	98	21	59·90	59·2	+ 0·7
β Tauri	5	17	26·67	26·69	− 0·02	61	30	54·08	51·8	+ 2·3
α Orionis	5	47	35·59	35·58	+ 0·01	82	37	21·95	21·2	+ 0·8
α Canis Majoris	6	38	58·66	58·63	+ 0·03	106	31	36·23	36·6	− 0·4
α Geminorum	7	25	39·31	39·29	+ 0·02	57	48	33·16	31·8	+ 1·4
			39·73	39·74	− 0·01			30·65	29·5	+ 1·2
α Canis Minoris	7	31	58·34	58·35	− 0·01	84	25	9·79	9·2	+ 0·6
β Geminorum	7	36	44·68	44·64	+ 0·04	61	38	21·25	20·3	+ 1·0
α Hydræ	9	20	42·44	42·43	+ 0·01	98	3	13·76	15·0	− 1·2
α Leonis	10	0	54·78	54·71	+ 0·07	77	21	0·62	59·4	+ 1·2
α Ursæ Majoris	10	55	3·40	3·51	− 0·11	27	29	39·12	38·6	+ 0·5
β Leonis	11	41	54·98	54·98	0·00	74	38	43·96	43·9	+ 0·1
β Virginis	11	43	24·13	24·20	− 0·07	87	26	47·35	47·7	− 0·4
γ Ursæ Majoris	11	46	27·06	27·25	− 0·19	35	31	37·22	37·2	0·0
α Virginis	13	17	49·27	49·29	− 0·02	100	25	45·54	46·8	− 1·3
η Ursæ Majoris	13	42	1·27	1·27	0·00	39	59	12·81	13·4	− 0·6
α Boötis	14	9	16·62	16·55	+ 0·07	70	5	13·40	12·8	+ 0·6
8 Libræ	14	42	56·87	56·89	− 0·02	105	24	46·90	47·1	− 0·2
α Libræ	14	43	8·31	8·32	− 0·01	105	27	27·41	27·8	− 0·4
β Ursæ Minoris	14	51	9·16	8·89	+ 0·27	15	16	20·65	21·2	− 0·5
α Coronæ	15	28	45·69	45·67	+ 0·02	62	48	42·96	42·5	+ 0·5
α Serpentis	15	37	22·44	22·43	+ 0·01	83	7	52·88	53·0	− 0·1
α Scorpii	16	20	49·70	49·70	+ 0·00	116	7	2·96	2·9	+ 0·1
α^2 Herculis	17	8	16·24	16·23	+ 0·01	75	26	52·77	53·2	− 0·4
α Ophiuchi	17	28	26·22	26·13	+ 0·09	77	20	6·26	5·4	+ 0·9
γ Draconis	17	53	21·34	21·35	− 0·01	38	29	36·01	35·2	+ 0·8
α Lyræ	18	32	11·92	11·89	+ 0·03	51	20	40·73	39·6	+ 1·1
γ Aquilæ	19	39	36·22	36·13	+ 0·09	79	43	31·05	30·7	+ 0·4
α Aquilæ	19	43	57·12	57·08	+ 0·04	81	29	55·32	54·5	+ 0·8
β Aquilæ	19	48	26·14	26·19	− 0·05	83	56	24·92	25·1	− 0·2
α^1 Capricorni	20	9	53·12	53·16	− 0·04	102	56	16·17	18·7	− 2·5
α^2 Capricorni	20	10	17·03	17·08	− 0·05	102	58	33·07	35·2	− 2·1

	Greenwich Mean Right Ascension.	Rad.	G.—R.	Mean N.P.D.	Rad.	G.—R.
	h m s	"	"	° ′ "	"	"
α Cygni	20 36 39·60	39·47	−0·13	45 13 6·23	7·3	−1·1
α Cephei	21 15 14·13	14·18	−0·05	28 0 24·59	24·8	−0·2
α Aquarii	21 58 35·50	35·49	+0·01	90 59 54·48	55·1	−0·6
α Piscis Australis	22 49 54·38	54·40	−0·02	120 21 48·31	47·2	+1·1
α Pegasi	22 57 47·33	47·33	0·00	75 32 50·49	50·7	−0·2
α Ursæ Minoris	1 8 3·05	2·37	+0·68	1 26 12·90	13·1	−0·2
δ Ursæ Minoris	18 17 30·19	30·60	−0·41	3 23 53·19	52·6	+0·6

The agreement, on the whole, between the Greenwich places and those of the *Tabulæ Reductionum* appears rather closer than with the *Radcliffe Observations*.

On the Resolvability of Star-Groups, regarded as a Test of Distance. By Richard A. Proctor, B.A.

There are considerations connected with the resolvability of star-groups which have not hitherto received much attention, so far as I am aware. They bear somewhat importantly on the opinions we are to form respecting the distribution of matter throughout the sidereal system.

In the first place, the resolvability of such clustering aggregations of stars as obviously form part of the sidereal system has been regarded as an important means of estimating the relative extension of different parts of that system. So long as a portion of such a clustering aggregation remains unresolved it has been assumed that the limits of the system in that direction lie beyond the range of the telescope which thus fails to effect resolution, and therefore that the extension of the system in that direction is far greater than in other directions where the same telescope shows the stars projected discretely on a perfectly black background.

In the second place, in the case of definite groups of stars, which either lie beyond the limits of the sidereal system, or if within those limits are yet separated from other parts of the system, and surrounded on all sides by relatively barren regions, it has been commonly assumed that we have, in the telescopic powers necessary to effect resolution, a means of forming a general estimate of the distances at which such groups may lie.

It is my purpose here to indicate certain considerations which point to opposite conclusions as respects both these cases.

If we were to accept the conclusion that where a portion of the galaxy is not resolvable with powerful telescopic aid, the sidereal system has a relatively great extension, it would follow from the smallness of the areas which many of these portions of the galaxy present that there is an extension of the system in *those directions* into long spike-shaped projections, lying in a di-

rection pointing exactly towards the solar system. When Sir William Herschel, for example, speaks of a region of this sort, of limited extent, which his great 40-feet reflector was unable to resolve, we must accept the conclusion that there is one of these spike-shaped projections extending (according to Sir William Herschel's own estimate) no less than 2300 times further into space than a line drawn from the Sun to *Sirius*. It is not only contrary to every law of probability that this is the real state of the case; but even if we could suppose that in this and in other similar instances such spike-shaped projections could *by mere accident* be directed along lines extending radially from the Sun, that is, if we could get over the argument from probability, there would still remain mechanical objections to our believing in such an arrangement. Knowing as we do that all the stars are in motion under the influence of their mutual attractions, and apparently also under the influence of some other and far greater forces adequate to generate the enormous observed motions, we ought scarcely to be willing to recognise in any part of the system a law of distribution which could not result from any conceivable dynamical processes.

It seems more reasonable to conclude that, where a cluster presents the peculiarity considered, there is not enormous longitudinal extension, but a real peculiarity of constitution; that, in fact, the observer has not been penetrating further and further into space as he increased his telescopic power, but simply analysing more and more searchingly a definite region of space.

In fact, Sir William Herschel, in one of his later papers, was led to consider this as perhaps the true explanation of the matter; for in 1817 we find him saying that his star guagings have in reality more direct reference to the condensation than to the distance of the stars, so that a greater number of stars in the field of view may be explained as well by a greater condensation of that portion of the galaxy, as by a greater extension of its figure in that direction in which the stars appear most numerous.*

Now as regards the case of a distinct cluster of stars, let us consider first the effect of distance on a group of stars all equal in magnitude and separated from each other by equal intervals. Supposing such a cluster so placed that the naked eye could recognise each separate orb, and then to sweep rapidly away into space, would it become nebulous or not before vanishing from view? As the group passed away each separate orb would grow less and less bright, and the distances separating orb from orb would grow apparently smaller and smaller. And clearly if these distances became too small to be distinguished, while the stars of the group yet continued visible, there would result a nebulosity of appearance. But suppose that, on the contrary, the stars of the

* It is rather surprising that in nearly all our treatises on astronomy the earlier papers by Sir Wm. Herschel receive far more attention than those he wrote when at the zenith of his fame. There is only one work I know of (Professor Grant's noble *History of Physical Astronomy*), in which Sir W. Herschel's labours are adequately represented.

group became invisible when the group was at such a distance that the intervals separating star from star would not be indistinguishable (if only the stars were brighter). Then clearly the group would vanish with increasing distance without ever becoming nebulous. Clearly also, if a telescope were employed to bring the retreating group into view, the same conclusions would hold good. A group which would become nebulous to the naked eye before vanishing would become so when examined under a telescope, let the telescope's power be what it might, while a group which would vanish without becoming nebulous to the naked eye would not become nebulous before vanishing under telescopic vision, whatever the telescopic power employed.

It is clear, then, that the nebulosity of a star group, whose members are equal and equally distributed, is a question not of distance merely but of constitution, of the relation between the size and brightness of the constituent orbs and the distances which separate them from each other.

But we may extend such considerations to the case of star-groups containing orbs of different orders of magnitude. Supposing a group of this kind to be passing away into space—as in the former case,—the question whether it would become nebulous at any stage or stages of its progress would depend on the question whether or not the order of stars about to disappear individually were congregated so closely that the eye could not distinguish the distances separating them. Clearly also it might be possible that an order of stars *not* about to disappear might present a nebulous appearance, in which case obviously all lower orders still remaining visible would be involved in that nebulous light. Such a cluster, in passing away from the eye, might also be nebulous at a certain distance, and become non-nebulous at a greater distance; all that would be necessary for such a result being that, while some of the lower orders of stars were distributed richly enough to present a nebulous appearance before vanishing, some of the higher orders should be so sparsely distributed as not to present a nebulous appearance before vanishing, or at any rate for some time after the lower orders *had* vanished.

It is further obvious that the same would be true if the retreating group were watched with a telescope of any power whatever (setting aside all question of the extinction of light in passing through space). The same appearances would be presented in precisely the same order when the group passed (star-order by star-order) out of the range of view of any telescope as when it passed out of the range of the unaided vision.

It follows that, if we apply telescopic power to a given group of stars, we can by no means conclude from the nebulosity of the group under such and such power that the group lies at such and such a distance, unless we are prepared to believe in the existence of certain laws of constitution to which all stellar clusters are subject. But such a belief is not likely to find acceptance with those *who are acquainted* with the observed variety in the constitution *of star-groups.*

It happens also that we have direct evidence that irresolvable nebulosity affords no proof of relatively enormous distance. When Sir John Herschel was surveying the neighbourhood of the lesser Magellanic Clouds he found that near the edge the Nubecula Minor was irresolvable with the 18-inch reflector, whereas the heart of this Nubecula could be clearly resolved.* Now it needs no proof that, if the Nubecula Minor (setting aside the nebulæ existing within it) were constituted of stars according to the generally uniform laws assigned to the constitution of the sidereal system, the centre of the Nubecula would be the part whose resolution would be most difficult. It is evident, therefore, that the outer parts of the Nubecula are constituted differently from the central region, and the possibility is suggested that the smaller stars seen in the central region belong in reality to the outer shell, whose real character is indicated by the irresolvability of the outer parts of the Nubecula's *disc* (as distinguished from the Nubecula's substance).

In this instance, then, it is distinctly proved that the irresolvability of a celestial region under Sir John Herschel's 20-feet reflector is no proof of relatively enormous distance. But what is thus proved for a certain telescopic power must be true of all telescopic powers. Hence, whatever the power may be under which a certain region appears nebulous, we have no proof that the stars contained within that region are further off than stars within a region resolvable under that power. But since this must be true of all powers, it must be true of naked-eye vision. Hence the stars forming the galaxy are not necessarily further off than those star-groups which the eye can resolve.

One important conclusion which is, I think, fairly deducible from what has been shown, is that, supposing a spiral of small stars such as I have suggested that the milky way may be, should extend along a part of its length, so far from the eye as to become invisible through distance, we ought not to expect that in passing from the visible part to this invisible portion all orders of resolvability down to utter irresolvability in the most powerful telescopes

* I quote the following passages from Sir John Herschel's *Notes on the Nubecula Minor*. They are all I can find which bear on the question of resolvability.

"The edge of the 'smaller cloud' comes on as a mere nebula."

"In the edge of the cloud vision bad, &c. . . . the cloud is not resolved, and seems a very mysterious object."

"We are now in *the cloud*. The field begins to be full of a faint light perfectly irresolvable."

"I should consider about this place the body of *the cloud*, which is here fairly resolved into excessively minute stars, which, however, are certainly seen with the left eye."

"Re-examined by the side motion the whole cloud, in general and in detail. The main body *is* resolved, but barely. I see the stars with the left eye. It is not like the *stippled* ground of the sky. The borders fade away quite insensibly, and are less or not at all resolved. The body of the cloud does not congregate *much* into knots, and altogether it is in no way a striking object apart from the nebula and clusters."

"Upper limit, *but here it is starry, at the other limit nebulous*."

ought to be recognised. On the contrary, this part of the spiral might exhibit in succession all the orders of change which the retreating group considered above was shown to be capable of showing (on a certain, not improbable, assumption as to its structure).

But my object is not so much to find evidence in favour of a special theory about a certain portion of the sidereal system as to indicate the varieties of appearance which are to be looked for in different parts of that system,—varieties which are, in fact, as likely to be met with (according to my views of the nature of that system) around the poles of the galaxy as in the richest portions of that wonderfully complex zone.

On an early Telescope made by Giuseppe Campani of Rome. By John Williams, Assistant Secretary.

At the sale of the late Dr. Lee's instruments, a few weeks since, I purchased an Italian telescope, which, appearing to be of considerable interest as an example of an early instrument by a then eminent maker, I trust I may be excused calling the attention of the Meeting for a few minutes to it.

On examination I found it to be one constructed by the celebrated Joseph Campani of Rome, who was considered as the best maker of telescopes of his day. Thus we find Cassini and other eminent Astronomers of that time employing instruments made by Campani in their astronomical researches.

I have been unable to find any satisfactory biographical account of this able artist, and can only ascertain that he flourished about the middle of the seventeenth century.

Weidler, in his *Historia Astronomiæ*, 4to, 1741, speaks very highly of Joseph Campani, and particularly notices his observations of *Saturn* and *Jupiter*, as well as his excellence as a maker of telescopes. As the quotation from Weidler is rather long and is in Latin, I shall content myself with the summary of his statement by Dr. Long, as given in his *Treatise on Astronomy*, 4to, 1785. His words are "About this time also (1641) Joseph Campani at Rome applied himself to the grinding of glasses for long telescopes, which far excelled all others of that time. Through the munificence of Louis XIV. Cassini had some made by this artist of 100 and 200 palms. Campani saw the shadow of *Saturn's* ring upon his body, as also his zones or obscure belts, and detected the shadows of *Jupiter's* satellites in passing over his body. It was with one of Campani's telescopes that Cassini first saw all the satellites of *Saturn*." Weidler adds, "Longiora quidem telescopia Campanus pauca fabricavit, in quibus, pro coloribus arcendis, tria vitra ocularia adhibuisse dicitur; attamen etiam breviora quæ composuit, 15, 20, 30 pedes longa, singulari perfectione prædita erant, ut ceteris similibus antecellerent." Which may be rendered, "*Campani also* made a few longer telescopes, in which to neutra-

lise colour, he is said to have applied three glasses in the eye-piece; and the shorter telescopes made by him of 15, 20, and 30 feet in length, were of such extraordinary perfection that they greatly surpassed all others of the same kind." It is also stated that "the Royal Academy (of France) are said to have had excellent telescope glasses of this artist's making which were of 200 and 300 feet focus and magnified four or five hundred times." In the *Speculum Hartwellianum* Admiral Smyth mentions this very instrument as "an old one deserving notice for its respectable age and its tolerable performance."

The telescope to which your attention is now called is one of the shorter ones fabricated by this excellent maker, it being but from 9 to 10 feet in length. The object-glass is 2 inches in diameter, reduced by a diaphragm to 1½ inch, and is ¼ inch in thickness, on it is written with a diamond "Giuseppe Campani in Roma," and the glass of which it is composed is of excellent quality. The eye-piece consists of three plano-convex lenses, the glass of which is of equally good quality, that nearest the eye being fixed and the other two reversible. There are inscriptions in Italian relating to these glasses, one of which reads as follows—"Quando questa parte sta dentro al cannello si vede l'oggetto piu chiaro," *i.e.* when this part is placed within the tube the object is seen clearer. The second is in the same words, excepting that "piu grande" occurs instead of "piu chiaro," implying that the power is increased. One of the Fellows of the Society (Mr. Buckingham), who is well acquainted with telescopes, has tested these powers, and estimates the lower one at about 20 and the higher at 25 times. Upon taking this portion of the eye-piece away altogether, the telescope becomes an astronomical one, and is much increased both in power and clearness. There is also on the eye-piece a nearly obliterated inscription evidently relating to its use for astronomical purposes, together with the letters PALI. XII., which appear to refer to the focal length as being 12 Palms. The Roman Palm is about 9 inches. This would give 9 feet as the focal length, which closely approximates to the truth. The case is composed of nine tubes formed of vellum, which are remarkably strong and light, the whole weight being but 2 lbs. 11 ozs. These tubes draw out in a manner similar to that employed in telescopes of the present day. When closed it is about 2 feet in length, but draws out to about 9 feet when in focus. The workmanship of the whole is exceedingly good.

When it came into my hands it had evidently not been drawn out for many years, and I had great difficulty in extending it to its full length. The glasses also were encrusted with the dirt of years, and required careful cleaning.

In the absence of a better test, I turned it on the word "Admiralty" on the opposite side of the quadrangle of this building (Somerset House), and was very favourably impressed with the sharpness of its definition and the flatness of the field. I have since viewed the Moon through it, and, considering the low power,

was much gratified, and indeed surprised, at the excellence of its performance; the lunar craters appearing, with all the powers, sharp and well defined, and the ragged edge very beautifully shown. The field of view is, however, small, and the instrument rather troublesome to use on account of its length, as, unless very carefully handled and adjusted, the flexure occasioned by the want of suitable support interferes somewhat with its efficiency. With the requisite care, however, it acts very fairly, and I have no doubt, if properly mounted, its performance would be very satisfactory.

Instrument for Sale.

A Transit Instrument, $3\frac{1}{2}$ feet focal length; object-glass, $2\frac{3}{4}$ inches aperture; brass Y's with agate bearings for fixing on stone piers; 3 eye-pieces; micrometer; level; clamp lamp, &c., but wanting the setting circle. Made expressly for a deceased Fellow of the Astronomical Society. Apply to Mr. Williams, Assistant-Secretary, Somerset House.

ERRATUM.

Page 152, line 7 from the bottom, "needle line" should be needle *hole*.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXX. *June* 10, 1870. No. 8.

William Lassell, Esq., President, in the Chair.

John Dickinson, Esq., 11 Upper Grosvenor Street, and
Capt. David Smith, Warkworth Terrace, Commercial Road,
were balloted for and duly elected Fellows of the Society.

On the Orbit of α Centauri. By E. B. Powell, Esq.

In the notes to my Second Catalogue of Binary Stars published in vol. xxii. of the Society's *Memoirs*, I gave a revised orbit for *α Centauri*, representing the results of observations up to 1862.3; and in a communication dated March 9, 1864, and published in the *Monthly Notice* for May of that year, I invited attention to the important part of the orbit about to be described, viz., the extremity of the perspective ellipse corresponding to the lesser maximum of distance. I have now to state that the comparison has, so to speak, doubled the above extremity, and that consequently the orbit can be determined very approximately. Hitherto it was impossible, as the lamented Captain Jacobs remarked, to say how far the apparent ellipse extended in a northerly direction, and correspondence between observation and calculation did not suffice to establish the correctness of a set of elements. Now, however, though even four or five additional years will enable us to improve the orbit, especially as to the time of periastral passage, the results I have arrived at undoubtedly approximate *pretty closely* to the truth. These results are as follows:—

Perspective Orbit.

Semi-axis major	$17''$
Semi-axis minor	$2''\cdot8$
Greater maximum distance	$23''\cdot8$
Position angle for ditto	$210^\circ\ 40'$
Lesser maximum distance	$10''\cdot4$
Position angle for ditto	$18^\circ\ 45'$
Greater minimum distance	$3''\cdot98$
Position angle for ditto	$301^\circ\ 45'$
Lesser minimum distance	$1''\cdot16$
Position angle for ditto	$115^\circ\ 30'$.

Real Orbit.

π	$38^\circ\ 40'$
e	$\cdot63944$
☊	$24^\circ\ 18'$
γ	$81^\circ\ 13'$
a	$20''\cdot13$
P	76·25 years
τ	1874·2

Without going into the details of my late observations, which will find place in a paper I shall do myself the pleasure of laying before the Society hereafter, I may mention that the position of *α Centauri* in the beginning of the year was as given below:—

Angle.	Distance.	Epoch.
$20^\circ\ 27'$	$10''\cdot24$	1870·1

The angle is the mean of 100 observations on thirteen nights; the distance, that of 162 observations on twelve nights. I feel great confidence in both measures.

I have to remark that, in the earlier orbits of *α Centauri* calculated by me, I was misled by a belief that Feuillée's observation was taken after periastral passage. Of late years it has become evident that the observation was taken before that passage. If the above mathematician's record can be fully relied on, as I imagine to be the case, it at once limits the periodic time to something less than 76·6 years. Feuillée remarks that, when he observed the star at Lima on July 4, 1709, the companion was west of the primary; but, about 1862·7, the companion crossed the meridian passing through the primary; consequently 1862·7–1709·5, or 2 (76·6) years, must be somewhat more than twice the period of *α Centauri*.

Madras,
April 20th, 1870.

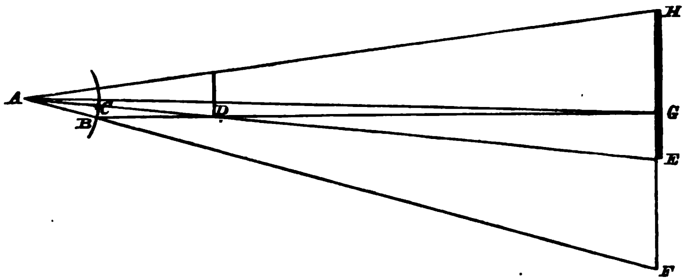
On the Determination whether the Corona is a Solar or Terrestrial Phenomenon. By George M. Seabroke, Esq.

It is my intention in this paper to attempt to show that, with the existing state of our knowledge of the corona, the theory set forth by Mr. Lockyer, that the corona is a terrestrial phenomenon, is quite possible, rather than to show that other theories are wrong,—and further to demonstrate how the question may be set at rest by observations on future eclipses. The points which present themselves are as follows:—

1. What are the facts with respect to the spectra of the corona seen in past eclipses?
2. What spectra ought we to obtain from the corona on the terrestrial theory during totality?
3. Are the spectra obtained from the corona in past eclipses reconcilable to those we ought to get on the above hypothesis?
4. What spectrum ought we to get from the corona after totality?
5. What spectrum ought we to get before totality on the following side of the Moon?
6. What difference will there be between the spectrum of the central portions of the corona and that of the distant parts during totality?

With regard to (1) during the Indian eclipse, Major Tennant writes,—"Directly I saw the whole Moon in the finder I set the cross-wires immediately outside its upper limb. By the time I got to the spectroscope the cloudy range seen in the photographs had vanished from the slit, and I saw a faint *continuous* spectrum. Thinking that want of light prevented my seeing the bright lines which I had fully expected to see in the lower strata of the corona, I opened the jaws of the slit and repeatedly adjusted by the finder, but without effect. *What I saw was undoubtedly a continuous spectrum, and I saw no bright lines.* There may have been dark lines of course, but with so faint a spectrum, and the jaws of the slit wide apart, they might escape notice." With respect to the American eclipse, Prof. Pickering, with an ordinary chemical spectroscope directed to the Sun's place during totality, saw a continuous spectrum with two or three bright lines; one "near E" and a second "near C." Prof. Young, while examining a part of the prominence at $+146°$, saw C, near D, a line at 1250 ± 20, and another at 1350 ± 20, and the 1474 K line very bright, but not equal to C and D_3; but he observed that the 1474 K line, unlike C and D_3, extended across the spectrum; and, on moving the slit away from the prominence, it persisted, while D_3 disappeared. He also believes that the two faint lines between it and D_3 behaved in like manner. On examining a prominence on the other side of the Sun, he observed nine lines and a faint continuous spectrum without any traces of dark lines in it.

As to the second point, let us find what spectrum we ought to obtain from a corona at a point on the Earth where the limbs of the Sun and Moon are in line; that is, where the eclipse is total, exactly.



Let A be a point on the Earth where Sun is eclipsed;
B C, limits of Earth's atmosphere;
D, the Moon;
H E, Photosphere of Sun;
E F, the apparent Corona.

Now, if the corona be terrestrial, the light producing it must be reflected or separated from the atmosphere within the triangle A B C.

Join B D and produce to G.

Then G is the most distant point from the limb on the Sun's disk, from which light is reflected to A by the atmosphere; and if the triangle E A F or angular extent of the corona from the Sun is given, we can find $\angle$ E A G.

The angles being small, $\frac{\angle \text{EAG}}{\angle \text{EAF}} = \frac{\text{GE}}{\text{EF}}$, approximately.

$$\text{GE} : \text{CB} :: \text{ED} : \text{DC}, \text{ therefore GE} = \text{CB}\,\frac{\text{ED}}{\text{DC}}, \qquad (1)$$

$$\text{and} \quad \text{EF} : \text{CB} :: \text{EA} : \text{CA}, \text{ therefore EF} = \text{CB}\,\frac{\text{EA}}{\text{CA}}, \qquad (2)$$

and E D = E A — A D, and A D being small in proportion to E A, E D may without great error be taken as equal to E A.

Dividing (1) by (2),

$$\frac{\text{GE}}{\text{EF}} = \frac{\frac{\text{ED or EA}}{\text{DE}}}{\frac{\text{EA}}{\text{CA}}} = \frac{\text{CA}}{\text{DC}} = \frac{\text{Height of atmosphere}}{\text{Dist. of Moon} - \text{height of atmosphere}},$$

$$\therefore \frac{\angle \text{EAG}}{\angle \text{EAF}} = \frac{\text{Height of atmosphere}}{\text{Dist. of Moon} - \text{height of atmosphere}},$$

$$EAG = EAF \frac{\text{Height of atmosphere}}{\text{Dist. of Moon} - \text{height of atmosphere}}.$$

If, for example, we now take

$$EAF = 30',$$

and Height of atmosphere $= 100$ miles,

and Dist. of Moon — ht. of atmosphere $= 240{,}000$ miles,

then $$\angle\, EAG = 30' \frac{100}{240{,}000} = 0''\cdot 75.$$

Therefore, the only part of the photosphere available in this case for illuminating the atmosphere is a ring of photosphere $0''\cdot 75$ in width, and from the figure it will be seen that only that part of the corona most distant from the centre (as at B) will receive even the whole of this light; and it is manifest from the figure that the nearer any part of the corona is to the centre (nearer C) the less light will it receive from the photosphere, so that the mean illumination of the corona by the photosphere is only equal to that which would be given by a ring $\frac{1}{2} \times 0''\cdot 75 = 0''\cdot 375$ wide.

Now, since the chromosphere extends from E towards F, the whole of the atmosphere producing the corona is illuminated equally by the chromosphere, and since the mean height of the chromosphere is much more than $0''\cdot 375$, or other height deduced from the foregoing formula, it is quite possible that the dark lines of the spectrum coming from so small an area of photosphere may be blotted out, as Mr. Lockyer observes, by the light from a greater area of chromosphere wherever the chromosphere contains the proper substances; and it is probable that the vapours of a number of substances from the photosphere are carried up into the chromosphere in small quantities sufficient to obliterate the dark lines, since we find the vapours of magnesium, sodium, barium, and iron, sometimes in the chromosphere.

Although the total amount of light of all kinds given by an equal area of chromosphere is small compared to that given by an equal area of photosphere, still each particular kind of light from the chromosphere is as intense, or nearly so, as that particular kind of light from the photosphere, so that, if equal areas of chromosphere and photosphere be illuminating a part of our atmosphere, that part would give a spectrum, having its dark lines erased by the chromosphere, or a continuous spectrum. When the area of the photosphere is much less than that of the chromosphere, the bright lines given by the chromosphere would be much more visible than the remaining dark lines of the photospheric spectrum.

From this it appears that during totality we ought to get from the corona a nearly continuous spectrum, with bright lines given by the substances in the chromosphere. Some of the dark lines

of the photospheric spectrum ought to remain where the chromosphere does not contain substances giving bright lines in their place. Where the illuminating areas of the photosphere and chromosphere are equal, which is possible where the chromosphere is unusually low, we ought to obtain a spectrum as above, but without bright lines, the chromospheric lines being then only just able to obliterate the dark ones.

3. In the Indian eclipse Major Tennant saw a continuous spectrum without bright lines, which is that we should obtain on the above hypothesis, when the areas of chromosphere and photosphere illuminating our atmosphere are equal; but it is shown above that during totality with the ordinary height of chromosphere the illuminating area of chromosphere is much greater than that of the photosphere, so that the part of the chromosphere illuminating that part of the corona under examination must have been unusually low, or, as was probably the case, there were bright lines, for, as he says the spectrum was very faint, they may have been missed. There ought on this hypothesis to have been dark lines, but Major Tennant says that with so wide a slit he might have missed them. Prof. Pickering saw a continuous spectrum with bright lines, which is what we ought to obtain when the atmosphere is illuminated by a greater area of chromosphere than photosphere, as has been shown to be the case when the chromosphere is at its normal height. The dark lines which ought to have been visible on Mr. Lockyer's theory might possibly have been too faint to be noticed, since, as stated above, the area of photosphere in this case would be small in proportion to that of the chromosphere, so that the bright lines would appear very plainly when the photospheric spectrum was too faint to render the dark lines visible. As to the 1474 K line observed by Prof. Young to extend across the spectrum beyond the other lines of the chromosphere, Mr. Lockyer observes that he often sees this line and often does not, which appears fatal to this being a real corona line, as, if so, it ought always to be visible. Prof. Young also seems, in the note to his observations, to be doubtful how far this line extended from the prominence; and it is very probable that this line is either iron or hydrogen. There seems to be no evidence that the other lines seen in the corona spectrum are not chromospheric lines.

4. With regard to this point, an inspection of the figure will show that, as the Moon passes over the Sun, more photospheric light becomes available for illuminating the corona; but so long as the available area of the photosphere is less than that of the chromosphere, the dark lines of the spectrum, due to the photosphere, will be erased by the chromospheric lines (wherever the chromosphere contains the proper substances), and as the Moon moves forward the spectrum should on this hypothesis change, and when the illuminating area of the photosphere becomes greater than the area of the chromosphere, the dark lines of the photospheric spectrum should appear. It will also be seen that the

larger the illuminating area of the photosphere becomes the smaller will be the difference between the spectrum of the interior part of the corona and that of the exterior part, since, whatever be the extent of the illuminating surface of the photosphere, the exterior parts of the corona will only receive an excess of light over that received by the interior part equal to the amount of photospheric light received by those parts during totality, or, as in the case above taken, the excess will be equal to that given by a ring of light from the photosphere $0''\cdot75$ wide (or G E in the figure), so that, when a few seconds of photosphere are visible to the observer, the difference between the spectra of the exterior and interior parts of the corona would be inappreciable.

5. What spectrum ought the corona to give before totality on the following side of the Moon? In this case, when the angular distance of the limits of the Sun and Moon is some seconds, the difference between the spectra of the exterior and interior parts of the corona is small, since no part of the atmosphere in this case will be illuminated by the photosphere, so we ought to obtain a chromospheric spectrum, together with a faint photospheric one caused by a small amount of photospheric light reflected from the photosphere by the chromosphere.

6. On the foregoing hypothesis during totality the parts of the corona nearest the centre should give a different spectrum to the more distant portions, since the portions nearer the centre receive less photospheric light than the more distant parts, and the same amount of light from the chromosphere.

In order to test the correctness of this theory, advantage may be taken of the following facts:—1st. At that period of the eclipse when the limb of the Sun and Moon are in line with the observer, there will be a difference between the central and distant parts of the corona, and this difference will decrease as the Moon passes on, whereas, by the other theory, there should be the same difference as long as the corona is visible. 2d. If the corona be terrestrial, the spectrum of any portion of it ought to be continually changing during the passage of the Moon; but, if solar, the spectrum should remain unchanged.

On the Displacement of the Bright Lines in the Spectrum of the Solar Chromosphere. By G. M. Seabroke, Esq.

The author in reference to a letter of Father Secchi to the Academy of Sciences of 25th April, writes,—

"For some time past, Mr. Lockyer has been kind enough to allow me the use of his telescope and spectroscope to prepare myself for the observations I intend making during the ensuing eclipse; and as I have been using the same spectroscope with which the discoveries in question have been made, and which have been contested by Father Secchi, I think it right to add my independent testimony on that point."

He remarks that the displacement of the bright lines of the spectrum of the chromosphere cannot be explained in the manner attempted by Father Secchi, by the rotation of the Sun, whatever the velocity of rotation is assumed to be. "For I have frequently seen a change of wave length in the same direction in the spectrum of prominences on opposite sides of the Sun, and if the change was produced by the Sun's rotation the change must be in opposite directions, since one side is approaching and the other receding from us. I also frequently see a change of wave length in the spectrum of one part of a prominence and not in another. How does Father Secchi's theory account for this? Besides, on his hypothesis, the bright lines of the prominences should never appear curved as I often see them, but should remain perfectly straight. The black lines should, on his hypothesis, be also displaced like the bright ones, so that the bright lines would still retain their position with respect to their corresponding black line, whatever be the velocity. Perhaps every one in eight of the prominences I have seen,—and I see two or three every time I looked at the Sun,—have given decided changes of wave length; in fact, the occurrence is so frequent, that unless any extraordinary change is seen, I make no note of it, and the changes of wave length are continually varying, and seldom last more than a quarter to half an hour, which clearly shows that the Sun's rotation has nothing to do with. That there are tremendous movements in the chromosphere is certain, from (1) the alteration of wave length observed in the space of a few minutes or sometimes seconds; (2) when a prominence is observed with a wide slit a change of form can generally be detected in a few minutes;" and he annexes rough sketches of the F line of two prominences showing change of wave length.

London,
June 1, 1860.

On a Spectroscope in which the Prisms are automatically adjusted to the minimum angle of deviation for the particular Ray under examination. By J. Browning, Esq.

In an ordinary spectroscope the prisms are usually adjusted to the minimum angle of deviation for the most luminous rays in the spectrum,—by preference I adjust them myself for the ray E in the solar spectrum. This being done, the prisms are screwed, or otherwise firmly clamped, to the main plate of the spectroscope. Thus adjusted they are liable to two sources of error, one of which places the observer at a serious disadvantage. First, only the particular ray for which the prisms have been adjusted, is seen under the most favourable circumstances, for only this ray passes, as all should do, through the train of prisms parallel to the base of each prism. Of more importance than this, however, is the

fact that the last prism of the train being fixed while the telescope through which the spectrum is viewed is moveable around an arc, it is only when the central portion of the spectrum is being examined, that the whole field of the object-glass is filled. By means of the models, as well as the instrument, which is before you, I hope to make myself clear on this point.

In figure 1, P P &c., represent a train of prisms adjusted as I have just described for the central portion of the spectrum, and screwed firmly in their places. T represents a telescope moving round a centre situated at K. In the position in which

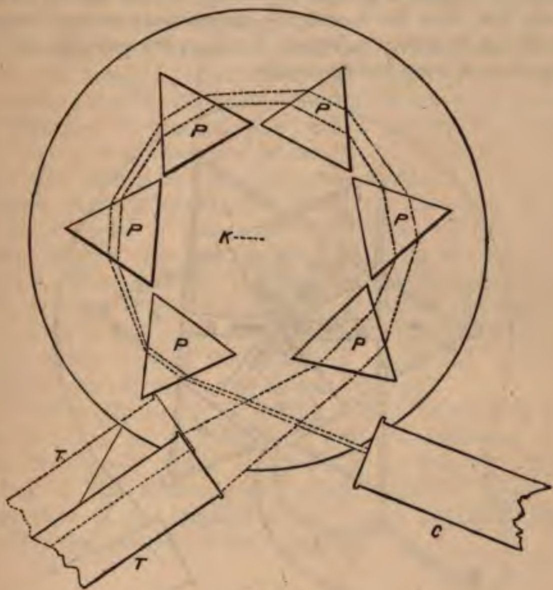


Fig. 1.

the telescope is placed, the whole field of the object-glass would be filled with the green light of the spectrum issuing from the last prism; but when the telescope is removed to the position shown by the dotted lines, either nearer to R or to V (in which case the red end or the violet end of the spectrum would be in the field of view), then, as you will see by the lines, only a small portion of the spectrum would fall on the object-glass. But it is obvious that owing to the deficiency in light at the extreme ends of the spectrum, it is just in these very positions that it is desirable that the whole field of the object-glass should be filled. Now this can only be effected when the prisms are adjusted to the minimum angle of deviation for the particular portion which is being examined of the spectrum; and this it will be if the adjustment I have spoken of has been correctly made. This

difference in adjustment is much more than is generally supposed, varying, in accordance with the refractive index of the glass employed, between 10° and 20° for the extreme portions of the spectrum.

Bunsen and Kirchoff, when making their celebrated map of the solar spectrum, adjusted the prisms they used (four in number) for the principal Fraunhofer lines; but the trouble of doing this is so great that few observers have ever seen the extreme portions of the solar spectrum under favourable circumstances.

A distinguished professor of natural philosophy informs me that he once adjusted a train of prisms for the line H in the solar spectrum, but that he found the experiment so troublesome that he should not be likely to repeat it unless for purposes of accurate investigation it were indispensable.

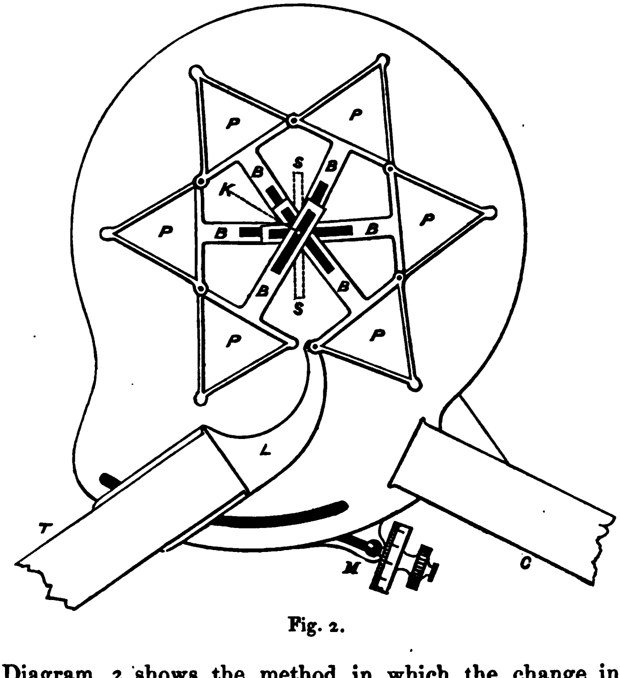


Fig. 2.

Diagram 2 shows the method in which the change in the adjustment of the prisms to the minimum angle of deviation for each particular ray is made automatically. In this diagram, P P &c., as before, represent prisms. All these prisms, with the exception of the first, are unattached to the plate on which they stand. The triangular stands on which the prisms rest are hinged together at the angles corresponding to those at the bases of the prisms. To each of these bases is attached a bar, B, perpendicular to the base of the prism. As all these bars are *slotted and run* on a common centre, the prisms are brought into

a circle. This central pivot is attached to a dovetailed piece of two or three inches in length, placed on the under side of the main plate of the spectroscope, which is slotted to allow it to pass through. On moving the central pivot the whole of the prisms are moved, each to a different amount in proportion to its distance in the train from the first or fixed prism, on which the light from the slit falls after passing through the collimator, C. Thus, supposing the first prism of the train of C, represented in the diagram, to be stationary, and the second prism to have been moved through 1° by this arrangement; then the third prism will have been moved through 2°, the fourth through 3°, the fifth through 4°, and the sixth through 5°.

Now for the contrivance by which this arrangement is made automatic. A lever, L, is attached by a hinge to the corner of the triangular plate of the last prism. This lever, by its further end, is attached to the support which carries the telescope, through which the spectrum is observed. Both the telescope and lever are driven by the micrometer-screw, M. The action of the lever is so adjusted that, when the telescope is moved through any angle it causes the last prism to turn through double that angle. The rays which issue from the centre of the last prism are thus made to fall perpendicularly upon the centre of the object-glass of the telescope, T, and thus the ray of light travels parallel to the bases of the several prisms, and ultimately along the optical axis of the telescope itself, and thereby the whole field of the object-glass is filled with light.

Thus the apparatus is so arranged that on turning the micrometer-screw, so as to make a line in the spectrum coincide with the cross-wires in the eye-piece of the telescope, the lever L, attached to the telescope and prisms, sets the whole of the prisms in motion, and adjusts them to the minimum angle of deviation for that portion of the spectrum.

Fig. 3.

Fig. 4.

Diagrams 3 and 4 represent the appearances presented when looking through the telescope from which the glasses have been removed. In diagram 3 it will be seen that the whole circle of the object-glass is filled with light, as I have just described, is the case with the new arrangement; while diagram 4 shows the

effect of moving the telescope through the angle in front of the fixed prism.

About three years ago I showed a rough model of the plan I have now described, to Mr. Gassiot, for whom I made the large instrument which was some time in use at the Kew Observatory. Mr. Gassiot immediately asked me if I would apply this arrangement to his large spectroscope. As I did not at that time see my way to make it self-acting by connecting the prisms with the micrometer-screw, I did not feel impelled to carry out the matter at once, owing to the fact that about this time the mapping of the solar spectrum with the large spectroscope in question was discontinued. I also felt that, with that munificent liberality for which Mr. Gassiot has distinguished himself, he had simply asked me to add this contrivance to his large and costly instrument in the interest of science generally, and not with any view to its immediate use.* Since this time, and particularly while I was constructing his solar spectroscope, Mr. Norman Lockyer has repeatedly urged upon me the desirability of completing the arrangement; and from the manner in which it has been received by the distinguished scientific men who have done me the honour to examine it minutely, I am induced to hope that its application will tend to facilitate further researches in spectrum analysis.

Note on the Alteration in the Colour of the Belts of Jupiter.
By John Browning, Esq.

In the Report of the Astronomer Royal to the Board of Visitors, there is the following remark:—

"There has been little opportunity of employing the instrument on other objects, except in a drawing of *Jupiter* by Mr. Carpenter.

"The comparison of this with drawings made eight or nine years ago appears to negative the idea of any change in the colour of *Jupiter's* belts."

With all possible deference to the Astronomer Royal and Mr. Carpenter, I beg to submit that a comparison of drawings made either this year or last year with others made eight or nine years ago, might not throw any light on this question. There is, as I have pointed out in my previous papers on this subject, some reason to believe that the change in colour in the equatorial belt of *Jupiter* is periodical. This belt of the planet at the time mentioned may have been of the same colour as during the last presentation,—that it was differently coloured during the presentation of 1868–9, is a fact attested by some six or seven, at least, well-known and skilful observers. It is true

* I am indebted to M. Gassiot for the opportunity of exhibiting the instrument on this occasion.

that in nearly every case these observers were using *reflectors*, of apertures varying between 6 and 12-inch, but I have heard from observers who have used achromatics varying between 4 and 8-inch, that they distinctly remarked the change in colour, although they do not seem to have seen it so plainly as those who have used reflectors. Unfortunately, none of these observers have published the results of their observations.

There is a singular agreement between observers who have described this phenomenon. The colour has been described as yellow, ochreish, brown, and tawny. All agree in ascribing this colour to the equatorial belt of the planet, ordinarily seen of a pure, or at most a pearly white.

The change has been one that might have been easily observed even by those who have not a quick eye for colour, by reason of the fact that the equatorial belt, which is ordinarily the brightest portion of the disk of the planet, and is so represented in every drawing I have ever seen of it, was, during the last presentation, much darker in colour than the bright belts nearer to the north and south poles of the planet.

Clapham, June 10th.

On the Proper Motion of Groombridge, 1830.
By W. T. Lynn, B.A.

In the year 1842 Professor Argelander announced* that he had detected that a star of the seventh magnitude on the boundaries of the constellations *Ursa Major* and *Canis Venatica* was animated by a Proper Motion considerably exceeding that known for any other star; amounting, in fact, annually to about 7″ of a great circle. This star had been observed by Groombridge, being numbered 1830 in his Catalogue of Circumpolar Stars, which was reduced to the epoch January 1, 1810, and edited by the Astronomer Royal, the date of publication being 1838. Having ascertained from Mr. Airy the exact times of Groombridge's observations, Argelander was able, the year after his discovery, to state† that the star's annual Proper Motion amounted to $+5''\!\cdot\!167$ in R.A. and $+5''\!\cdot\!70$ in N.P.D. This satisfied within the limits of probable error of observation all the other positions observed by Lalande, Bessel, Argelander, and Nicolai.

It occurred to me recently that, as a large number of very accurate observations of this star had been made at the Royal Observatory, Greenwich, since the year 1843, in which Professor Argelander made this determination, and that these observations extended now over more than a quarter of a century, it might be worth while to put them together, with the view of ascertaining whether the large Proper Motion in question continues uniform.

* *Astronomische Nachrichten*, No. 455. † Ibid. No. 475.

This comparison I now offer to the Society; remarking that, by the kind permission of the Astronomer Royal, I am enabled to include the result of the whole of the positions contained in the New Seven-year Catalogue of 2760 Stars, which is now passing through the press, and also that derived from the observations made last year, which are still in manuscript. In bringing back the observations to the 1st of January of the year which forms the epoch of each catalogue, the proportional part of the proper motion, as given by Argelander and copied into the British Association Catalogue, has been carefully applied. This had not indeed been done in the 12-year and 6-year Catalogues; but at the end of the 7-year Catalogue for 1860, published as an Appendix to the *Greenwich Observations* for 1862, will be found (pages {cxxi.} and {cxxiii.}) the positions which would have been given in those catalogues, had this been done.

Positions of *Groombridge*, 1830.

Epoch.	R.A.	No. of Obs.	N.P.D.	No. of Obs.	Authority.
	h m s		° ′ ″		
1845	11 44 1·53	13	51 10 10·90	16	12-year Catalogue.
1850	11 44 19·04	17	51 12 20·04	19	6-year Catalogue.
1860	11 44 53·91	18	51 16 37·44	8	7-year Cat. (1860).
1864	11 45 7·83	8	51 18 21·22	8	7-year Cat. (1864).
1869	11 45 25·24	2	51 20 29·84	2	Gr. Obs. 1869, MSS.

From this we obtain the following mean annual variations for the years comprised between each successive epoch:—

Years.	Ann. Var. in R.A.	Ann. Var. in N.P.D.
	s	″
1845–1850	+3·502	+25·83
1850–1860	3·487	25·74
1860–1864	3·480	25·94
1864–1869	+3·482	+25·72

If we subtract from each of these the annual precession, which in R.A. is $+3^{s}{\cdot}144$, and in N.P.D. $+20''{\cdot}01$, we find finally for the annual Proper Motion in each interval the following values:—

Years.	Proper Motion in R.A.	Proper Motion in N.P.D.
	s	″
1845–1850	+0·358	+5·82
1850–1860	0·343	5·73
1860–1864	0·336	5·93
1864–1869	+0·338	+5·71

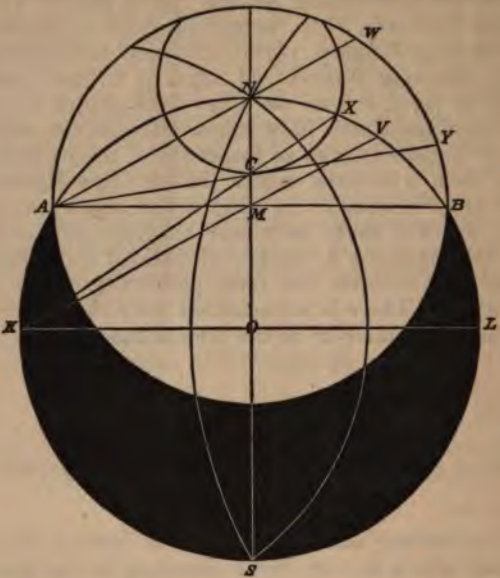
The constancy, or nearly so, of the Proper Motion of this star, would seem, therefore, to be established.

The Society have received from Mr. F. Abbott a communication, entitled "Some further Observations on the Variable Star *η Argûs* and the Surrounding Nebula," dated Hobart Town, Tasmania, 15th February, 1870, accompanied by a drawing of the nebula taken 28th January, 1870. He refers to his former drawings, 1863, *Monthly Notices*, vol. xxiv. p. v., and 1868, ditto, vol. xxviii. p. 200, and to the *Cape Monograph*, and remarks that the alterations which have taken place in the nebula since 1868 will be at once seen on an inspection of the drawings and by comparing them with each other. In the *Cape Monograph* the dark space is an inclosure. In 1863 it had two openings, one at each end. In 1868 there were four openings, and now in 1870 there are five, exposing a number of isolated and distinct stars, which render it difficult but from position to know which is the star *η Argûs*. There is attendant on these changes an increase of light which is notable up to the present time.

On a Property of the Stereographic Projection. By Prof. Cayley.

I am not aware whether it has been noticed that the very same circles which in the direct stereographic projection of a hemisphere (viz., that wherein the projection is on the plane of a meridian) represent the meridians and parallels respectively,—represent also in the oblique projection of the hemisphere meridians and parallels respectively. In fact, in the direct projection where the poles N, S, are in the horizon-meridian, or bounding circle of the projection, if we take a chord A B at right angles to N S, and on A B as diameter describe a circle, the original (meridian and parallel) circles will, as the appearance of the figure at once suggests, represent meridians and parallels in the oblique projection in which the horizon or bounding circle of the projection is the circle diameter A B, and where consequently the North Pole N is brought into view, the South Pole S being beyond the limits of the projection. That this really is so, is clear from the consideration that in any stereographic projection whatever, the meridians will be circles passing through two fixed points N, S, and the parallels be circles cutting the meridians at right angles. (Or, what is the same thing, the parallels also pass each of them through two fixed imaginary points, the antipoints of N, S, but this in passing.) And, moreover, since in the oblique, as well as in the direct projection, the longitude of any meridian, as reckoned from the central meridian N S, is the angle at N between the two meridians, the longitude for a given meridian is the same in the two projections respectively. But the co-latitudes are not the same in the two projections respectively; viz., a circle which in the

direct projection represents the parallel co-latitude c, will in the oblique projection represent the parallel of a different co-latitude



c'. The relation between the values of c, c', will of course depend upon the position of the bounding circle A B of the oblique direction: to define this position, we may use either the arc N M which in the direct projection determines the co-latitude of the centre M of the oblique projection (say $\text{N M} = \Delta$, that is, $\text{N V} = \Delta$), or by the arc $\bar{\text{N}}$ M which in the oblique projection determines the distance of N from the centre, or co-latitude of the centre (say $\text{N M} = \Delta'$, that is, $\text{B W} = \Delta'$.) The obliquity in the oblique projection is thus $90° - \Delta'$, viz., this is the inclination of the plane of projection to that of the horizon-meridian in the direct projection. We have also $c = \text{N X}$, $c' = \text{W Y}$. The relation between the angles Δ, Δ', is easily found to be

$$\tan \tfrac{1}{2} \Delta = \tan^2 \tfrac{1}{2} \Delta',$$

viz., taking the radius in the direct projection to be $= 1$, we have

$$\text{O M} = \tan \tfrac{1}{2} (90° - \Delta),$$

$$\text{M A} = \sqrt{1 - \tan^2 \tfrac{1}{2} (90° - \Delta)},$$

$$\text{M N} = 1 - \tan \tfrac{1}{2} (90° - \Delta);$$

wherefore

$$\sqrt{1 - \tan^2 \tfrac{1}{2} (90° - \Delta)} \, . \, \tan \tfrac{1}{2} \Delta' = 1 - \tan \tfrac{1}{2} (90° - \Delta).$$

and thence

$$\tan^2 \tfrac{1}{2} \Delta' = \frac{1 - \tan \tfrac{1}{2} (90^\circ - \Delta)}{1 + \tan \tfrac{1}{2} (90^\circ - \Delta)} = \tan \tfrac{1}{2} \Delta,$$

the required relation.

We have moreover

$$\begin{aligned} NC = 1 - \tan \tfrac{1}{2} (90^\circ - c) &= AM \left\{ \tan \tfrac{1}{2} \Delta' - \tan \tfrac{1}{2} (\Delta' - c') \right\} \\ &= \sin \Delta' \left\{ \tan \tfrac{1}{2} \Delta' - \tan \tfrac{1}{2} (\Delta' - c') \right\} \\ &= 2 \sin^2 \tfrac{1}{2} \Delta' - \sin \Delta' \tan \tfrac{1}{2} (\Delta' - c'), \end{aligned}$$

that is

$$\begin{aligned} \tan \tfrac{1}{2} (90^\circ - c) &= \cos \Delta' + \sin \Delta' \tan \tfrac{1}{2} (\Delta' - c') \\ &= \frac{\cos \tfrac{1}{2} (\Delta' + c')}{\cos \tfrac{1}{2} (\Delta' - c')}, \end{aligned}$$

or, what is the same thing,

$$\frac{1 - \tan \tfrac{1}{2} c}{1 + \tan \tfrac{1}{2} c} = \frac{1 + \tan \tfrac{1}{2} c' \tan \tfrac{1}{2} \Delta'}{1 + \tan \tfrac{1}{2} c' \tan \tfrac{1}{2} \Delta'},$$

that is

$$\tan \tfrac{1}{2} c = \tan \tfrac{1}{2} \Delta' \tan \tfrac{1}{2} c',$$

which is the required relation between c and c'. In the particular case $\Delta = \Delta' = 90^\circ$, the two projections coincide, and we have, as we should do, $c' = c$.

Note respecting Solar Spots visible to the Naked Eye. By A. R. Hill, Sub. Lieut. R.N.

On Sunday, 22[d] May, at from about 5 to 6.30 P.M., the extreme light of the Sun being obscured by a peculiar scud drifting over it, and giving the whole disk a reddish appearance, with the borders less luminous than the centre, I observed three large spots, A, B, and C, as in diagram, fig. 1, distinctly visible with the naked eye, especially A and B, A being the most distinct, and C being only visible at times, when the ardour of the Sun's rays was diminished more than at others.

On Monday, 23d, at about the same time P.M., the atmosphere being still in the same state as on the previous day, but more advantageous for naked-eye observations, I observed the spots, A, B, and C again, C being more distinct than on the previous evening; and also another spot, D, fig. 2.; but this latter being only visible at the most advantageous intervals.

During these two days the barometer averaged about 30·20 in. and thermometer, Fahrenheit, 66° in the shade at the times of observation.

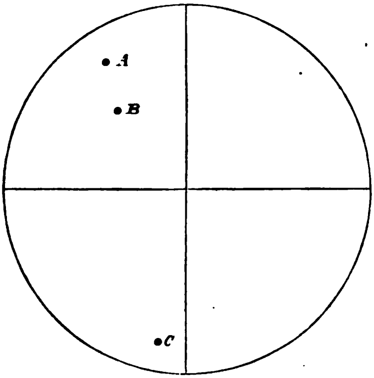


Fig. 1.

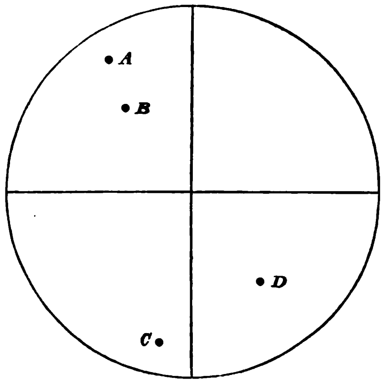


Fig. 2.

Norton, Presteign, Radnorshire,
May 30th, 1870.

Winnecke's New Comet.

(*Extract of a Letter from Dr. Winnecke to Mr. Hind, dated Carlsruhe, May* 31.)

"You have probably received through the Vienna Academy the news of a small comet discovered by me in the night, May 29–30, in *Pisces.*

"I can send you to-day the following observations:

			Comet – Star.	
May 29	$14^h\ 12^m\ 38^s$	M.T. Carlsruhe	$\Delta\alpha = +\ 0^m\ 13^s{\cdot}55$	8 Comp.
	$14\ \ 13\ \ 22$	,,	$\Delta\delta = +\ 0'\ 9''{\cdot}9$	5 Comp.

The star of comparison is only to be found in the *Bonner Durchmg.* 9.3, 1855·0.

$$\alpha = 0^h\ 47^m\ 55^s{\cdot}9 \qquad \delta = +\ 29^\circ\ 1'{\cdot}5$$

The last night was very cloudy, so that I have got but 3 comp. with a star twice observed by Argelander, *Bonn. Beob.* VI. + 28° No. 159.

	M.T. Carls.	Comet's R.A.	Comet's Decl.
May 30	$14^h\ 13^m\ 34^s$	$= 0^h\ 50^m\ 9^s{\cdot}55$	$= +\ 28^\circ\ 52'\ 18''$

The comet is a round, pretty bright nebula, of about 2½ minutes in diameter"

Elements and Ephemeris of Winnecke's New Comet.

(Communicated in a Letter from Dr. Winnecke to Mr. Hind.)

The elements, which depend upon observations at Carlsruhe to June 2, and one made by Prof. Argelander on June 5, are as follow :—

$$T = 1870, \text{July } 12{\cdot}905, \text{ M.T. at Berlin.}$$

$$\left.\begin{array}{rl} \pi = & 302^\circ\ 14'\ 53'' \\ \Omega = & 140\ \ 3\ 45 \end{array}\right\} \text{Apparent Equin. June 2.}$$

$$i = 57\ 19\ 17$$

$$\log q = 9{\cdot}99579$$

Retrograde.

Ephemeris for 12^h, Berlin.

1870.	R.A. ° ′	δ ° ′	Log r.	Log Δ.
May 30	12 30·7	+28 53·8	0·0910	0·2078
June 3	13 42·7	27 51·0	0·0775	0·1779
7	15 0·6	26 37·2	0·0643	0·1445
11	16 26·9	25 8·9	0·0517	0·1070
15	18 4·4	23 21·0	0·0398	0·0649
19	19 58·1	21 6·6	0·0289	0·0173
23	22 14·9	18 14·4	0·0192	9·9632
27	25 5·3	14 27·2	0·0109	9·9018
July 1	28 46·5	9 16·1	0·0042	9·8328
5	33 45·7	+ 1 59·0	9·9994	9·7571

Observations of Winnecke's New Comet. By S. J. Perry, Esq.

The comet discovered on May 30, at Carlsruhe, by Dr. Winnecke, was observed here on June 6, at about $12^h\ 30^m$ G.M.T. Its brilliancy must be considerably greater than that of the comet found last year by the same astronomer. Since it was picked up almost immediately with a low power, although the light of both Sun and Moon were sufficiently strong to illumine the field distinctly, and the object was not many degrees from the eastern horizon.

The co-ordinates corrected for instrumental errors and refraction were on June 6, at $17^h\ 45^m\ 38^s{\cdot}5$ sidereal time.

R.A. $0^h\ 57^m\ 3^s{\cdot}6$ N.P.D. $62^\circ\ 49'\ 31''{\cdot}6$

Light clouds prevented my obtaining a position of June 7, and last night the sky was completely obscured by clouds. I hope to observe it again when the Moon rises after midnight. I have several times swept in vain for D'Arrest's comet.

June 8, 1870,
Stonyhurst Observatory, Whalley.

Instrument for Sale.

A Dip Sector or Dipping Needle, German make, in a handsome case, quite new, never having been used, provided with a mirror, by which the deviations of the magnetic needle are indicated with the greatest possible accuracy. For further particulars apply to the Assistant-Secretary at the Rooms of the Society.

ERRATA.

Page 176, line 2 from bottom, *for* "richly" *read* "sickly."

Page 179, line 8 from bottom, *for* Feb. 2, *read* Feb. 10.

„ line 7 from bottom, *for* $9^h\ 7^m\ 14^s{\cdot}9$, G.M.T., *read* $9^h\ 8^m\ 10^s$, G.M.T.

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~~Incomp~~ Complete

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Total Eclipse of the Moon, 1870, *July* 12. By the Astronomer Royal.

I made no observations of the eclipse, except in regard to the intensity of illumination of the Moon's disk at the middle of the eclipse. I remarked as follows :—

1. The difference in the intensity of illumination on the northern portion of the disk (least illuminated) and the southern portion (most illuminated) was much greater than I expected to see in an eclipse in which the Moon passed so near to the centre of the shadow.

2. With my eyes unarmed, being extremely short-sighted (the distance for distinct vision less than 5 inches), and every luminous object being seen as a broad blurr, I can compare the quantity of light from sources of very different character. Thus I found that the quantity of light received from the Moon was less than that from *Saturn* and greater than that from α *Aquilæ*, including some from β and γ *Aquilæ*.

3. The light of the Moon increased considerably in ten minutes after the time of central eclipse.

4. I infer from Nos. 1 and 3 that the only part in which the shadow is nearly total is the centre of the shadow, and that a large amount of light falls within the geometrical limits of the true shadow.

July 18, 1870.

On the Lunar Eclipse, 12th July, 1870.
By C. H. Weston, Esq.

I had hoped, with an Observatory commanding from its elevated position an angle of more than 90° declination, and an uninterrupted eastern horizon belonging to a circle of 120 miles in circumference, to have well witnessed the " First contact with the Shadow" on the Moon's Eastern Limb, north of her equator, and near the Ringgebirg *Olbers*. A fixed bank of clouds, however, prevented any observation until the Moon had risen about 8°, when the Earth's shadow was found to have already covered *Hevel, Cavalerius*, and parts adjacent to *Reiner*, and had reached the resplendent cliffs of *Aristarchus*, and the almost equally brilliant highlands between *Aristarchus* and *Herodotus*.*

At this time the whole of the western edge of the shadow was dark, while the east periphery of the Moon was brightly visible. In 10 minutes the shadow north of the equator had reached the base of the great *Copernicus* and districts not far east of *Tycho* (including part of his radial system) on the south; then passing *Eratosthenes*, it touched the southern extremity of the mountain-range of *Apenninus* three minutes later.

After an interval of six minutes the extensive surface, including west of *Sinus Iridum* on the north, and those of the more southerly *Archimedes*, together with the greatest part of *Mare Nubium* and *Tycho* on the south, was observed in shadow.†

At this time the east periphery was reddish, and the western edge of the shadow orange, resulting from the combination of the red tinge and the yellow lunar light beneath. About six minutes later the shadow had reached the districts of the *South Pole*, and near the eastern part of *Mare Serenitatis*. Before the Moon had been so far obscured the *Sinus Iridum* was visible, but after reaching the last described limits all the shaded surface showed its details—the absence of light allowing perhaps a greater dilatation of the pupil of the eye. Now *all* the eclipsed parts were reddish, and the western margin of the shadow purplish.

After 15 minutes the shadow reached as far as the western edge of *Mare Serenitatis* and the eastern portion of *Mare Tranquillitatis*, and on the north, near *Hercules*, and not far from *Endymion*. In two minutes the eastern margin of *Mare Fecunditatis* was in-

* The telescopes used were Newtonians, 9 and 14·25 inches and 9 and 16 feet respectively — the former on the back of the latter — on the same equatoreal mounting, and thus most readily available for comparative observations.

† During the eclipse, and some days before and after full moon, the region of *Archimedes* was well seen. Besides this Ringgebirg there were two others visible, nearly as large in circumference, but apparently less in elevation, than *Archimedes*, more resembling incipient rings common on other parts of the lunar surface. These were clearly the "incipient or low protrusions, *circular and rugged*," mentioned in my paper of December last, and but indistinctly *seen* in the earlier phases of the Moon.

volved, and two minutes later the eastern part of *Mare Crisium* and the Ringgebirg *Geminus* further north. At this time the bright western crescentic limb of the Moon, contrasted with the eclipsed portions, afforded a fine example of the enlarging effects of irradiation. The disturbed district between *Mare Crisium*, *Mare Tranquillitatis*, and *Mare Fecunditatis*, were finely brought out just before entering the penumbra, and *Proclus* (nächst *Aristarch* das hellste Ringgebirg, B. & M.) was strikingly prominent.

After an interval of five minutes the shadow extended to the west of *Mare Fecunditatis*, and nearly west of *Mare Crisium*, and about three minutes later the phase was total.*

Now light appeared gradually to extend itself eastward over the lunar surface, and in about eight minutes after the total eclipse the converging ray-system of *Tycho* on the south, and all the country east and west of *Sinus Iridum* on the north, came out to view, while the region about the first impact was still darkish. In about 40 minutes the light was more uniformly diffused over the Moon, and after quarter of an hour the illumination was pretty equal on the east, and west, and south, while the north was darkest. After 10 minutes the east side was the brightest. Small star near the S.W. limb was well seen.

After six minutes the south-eastern periphery was brightest. *Tycho's* circular structure loomed out. The Moon gradually became darker after the mid-phase, and then the western parts also gradually less visible. *Aristarchus* was a very brilliant object.

Soon the reddish tint on the north-west appeared, which in about four minutes extended over the surface towards the south-east. Then came symptoms of approaching dawn, and two minutes later the north-east above *Grimaldi* began to brighten. Then the concave band of solar light appeared on the lunar disk, passed over *Grimaldi* and the disturbed regions east of, but distant from, *Gassendi*, and in four minutes illuminated the south-east of *Mare Humorum*. Clouds now began to cross the Moon.

Later (six minutes) the light dawned upon *Cichus* (eine grosse Vertiefung, B. & M.), and further south to the south-east of *Tycho*. More clouds. Later obtained a glimpse, and found the light had reached *Tycho*. Another 20 minutes, and the west of the north end of the range of *Apenninus* was illumined. Nothing afterwards was visible.

During the progress of the eclipse the advancing convex shadow of the Earth was observed to produce on the Moon's spherical surface a series of arcs, not *concentric* but *convergent* towards the north periphery; and it should be noted that the returning light impinged first at a point not identical with the shadow's first contact (which must have been near *Olbers*), but at a point a little

* The region between *Schubert* and the western periphery of the Moon (the last portion involved in shadow) was apparently more extensive than in Mean Moon, and doubtless arose from her libration, the maximum of which would occur two days later.

north-east of *Grimaldi*, several degrees further south than *Olbers* —visible effects of the different relative positions and motions of the Earth and Moon during the period of the eclipse.

Ensleigh Observatory,
Lansdowne, Bath, July 1870.

On Mr. Browning's Spectroscope. By M. C. de Littrow.

(*Note addressed to the Foreign Secretary.*)

I take the liberty to let you know that the essential idea in Mr. Browning's arrangement of the spectroscope (*Monthly Notices*, vol. xxx. p. 198) has already been executed and published by my deceased son Otto, in the year 1862, as you will find in the annexed treatise (*Proceedings of the Imperial Academy of Sciences of Vienna*, vol. xlvii.). The only difference between the two instruments consists in the sparing of the second telescope, which my son has effectuated by placing a mirror behind the last prism, so that the same telescope serves also as a collimator. Besides this advantage, he obtained thus the double effect of the employed prisms. He enclosed the whole apparatus in its box, so that he needed not darken the room in which he experimented.

I beg the favour of bringing this short note to the knowledge of the readers of your *Monthly Notices*.

Vienna, July 18, 1870.

Note on the Automatic Spectroscope. By John Browning.

When constructing the automatic spectroscope which I have recently had the honour of describing to the Society, I saw the desirability of giving a motion to the telescope without the use of a cam. Having spoken to Mr. Proctor on this subject, he told me that he had been convinced, while the new spectroscope was under discussion at the last meeting, that my principle really involved the complete solution of the problem of securing minimum deviation for all orders of light-waves. It was only necessary for the purpose that the slot movement should be extended to the first prism, and to the viewing telescope. It is obvious that in this way the necessary amount of motion will be communicated to the telescope, and also to the first prism, which in the former arrangement was a fixture. I am making an instrument containing this improvement, and hope to have the honour of exhibiting it at the next meeting of the Society in November.

111 *Minories, July* 2, 1870.

Note on Mr. Browning's Automatic Spectroscope.
By Richard A. Proctor, B.A.

It occurred to me, while Mr. Browning's spectroscope was under discussion at the last meeting of the Society (and more particularly when Prof. Pritchard raised the question whether all rays pass through the battery with minimum deviation for each prism), that by a slight modification Mr. Browning's ingenious device was capable of more completely fulfilling the conditions of the problem suggested by Prof. Pritchard. I am not sure that practically the spectrum will be viewed under much more favourable conditions by the plan I am about to propose; but fortunately the plan has (Mr. Browning tells me) other advantages. I need hardly say that I only present it as a modification of his most ingenious plan, and claim no separate merit for the new arrangement, even though the optical performance of the battery of prisms should be appreciably improved by it.

It is necessary to premise that although it is possible, as I shall show, to secure minimum deviation for rays of all degrees of refrangibility, yet we can obtain only an approximation to that condition which minimum deviation is intended to secure. Even in the case of a single prism, the primary and secondary foci of the emergent pencil corresponding to rays of given refrangibility are not absolutely coincident when such rays pass through the prism with minimum deviation. If ϕ and ψ' represent the angles of incidence and emergence, ϕ' and ψ the angles of first and second refraction, we have for the distance between the primary and secondary foci of the emergent pencil the expression

$$\left[\frac{\cos^2\phi'\cos^2\psi}{\cos^2\phi\cos^2\psi'}u + \frac{t\cos^2\psi}{\mu\cos^2\psi'}\right] - \left[u + \frac{t}{\mu}\right]$$

where u represents the distance of the origin of the source of light from the point of incidence, t the length of the path within the prism, and μ the refractive index of the prism for the rays considered. Hence, that the primary and secondary foci of the emergent pencil may coincide, we must have

$$(\cos^2\phi'\cos^2\psi - \cos^2\phi\cos^2\psi')\,\mu u = t\cos^2\phi\,(\cos^2\psi' - \cos^2\psi).$$

Only when t vanishes does this reduce to the condition

$$\cos^2\phi'\cos^2\psi = \cos^2\phi\cos^2\psi'$$

or

$$\phi = \psi',$$

which is the condition of minimum deviation.

Now t is ordinarily of appreciable length for each prism, in the case of a battery of prisms; and therefore the resulting distance between the primary and secondary foci is also appreciable, though small, even in the case of the first prism of the battery,

and will increase with each successive prism. The expression for this distance reduces, in the case of minimum deviation, to

$$\frac{t}{\mu}(1-\mu^2)\tan^2\psi',$$

or, if the refracting angle of the prism be $2\,i$, to

$$\frac{t}{\mu}(1-\mu^2)\tan^2 i.$$

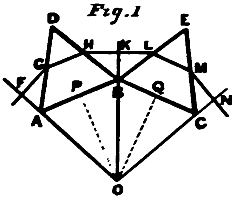
The circle of least confusion will therefore have a radius represented by

$$\frac{t\lambda(1-\mu^2)\tan^2 i}{\mu x\left[\sqrt{1-\mu^2\sin^2 i}+1\right]+t\left[(1-\mu^2\sin^2 i)^{\frac{3}{2}}+1\right]}$$

where λ is the breadth of the pencil at the place of emergence. It is obvious, however, that this expression will be minute under the ordinary conditions of spectroscopic observation.

Setting the condition of minimum deviation, instead of the true optical condition, as that which we are to aim at, it is obvious that in the case of a battery of prisms, all of equal refracting angle, what we require is that, first, the bases of all the prisms shall always circumscribe a circle; and, secondly, that the angle of incidence on the first surface of the first prism shall be equal to the angle of emergence from the second surface of the last prism. If these conditions are fulfilled, the condition for minimum deviation must necessarily be fulfilled; if they are not fulfilled, there cannot be minimum deviation.

Fig. 1



Suppose, for instance, that A D B and B E C (fig. 1) are two prisms of equal refracting angles, D and E, and connected, as in Browning's spectroscope, at the angle B; and let F G H L M N be the path of a ray refracted with minimum deviation through each. Then

$$\cos B H K = \mu \sin\frac{D}{2} = \mu\sin\frac{E}{2} = \cos B L H;$$

that is, B H L is an isosceles triangle, and O B K, the bisector of the angle D B E, is perpendicular to H L.

Now P O and Q O, the rectangular bisectors of A B and B C, meet on B O, as at O; and O A F makes with G A the angle G A F, equal to the angle H B K. But the angle A G F is equal to the angle B H K. Hence the angle A F G is equal to the angle B K H, or is a right angle.

In like manner O C N is at right angles to M N.

If another like prism be added at C, the same reasoning will apply; and so on, whatever be the number of prisms.

It follows that the lines from a single point O to the attached angles A, B, C, &c. of any such series of prisms are perpendicular to the course of the ray between the prisms. But any two perpendiculars O P and O Q from this point O on successive base-lines A B and B C are obviously equal; hence all such perpendiculars are equal; and therefore A B, B C, &c. are all tangents to a circle about O as centre. Further

$$\angle \text{ FGA} = \angle \text{ BHK} = \angle \text{ BLH};$$

that is, the angle of incidence on any prism of the series is equal to the angle of incidence on the next. Therefore the angle of incidence on the first prism is equal to the angle of incidence on the last, and therefore to the angle of emergence from the last.

These are the conditions, then, which must be fulfilled in all positions of a self-adjusting battery, if rays of all degrees of refrangibility are to pass with minimum deviation through each prism, viz.

1st. *All the bases must be tangents of a circle.*

2dly. *The pencil must emerge from the last prism at the same angle at which it fell on the first prism.*

The first condition Mr. Browning's plan obviously fulfils, for the perpendiculars bisecting the equal bases all pass through one point.

The second condition the plan does not, as first devised, accurately fulfil, because the first prism is fixed, and the incident white light falls on it from the slit at a constant angle; so that there can be minimum deviation only for rays of a certain degree of refrangibility.*

The modification I suggest consists merely in fixing an angle (the angle corresponding to A in fig. 1) of the first prism, and causing this prism to move so as that the line corresponding to O A F shall be constantly perpendicular to the axis of the incident pencil F G. Since the axis of the incident pencil is fixed in position, this involves only the addition of a fixed slot in the position O A F.

It further occurred to me that since by this arrangement the axis of the emergent pencil from the last prism is situated (as M N in fig. 1) in a direction at right angles to the line from O to the angle corresponding to C, all that is necessary to keep the viewing telescope in its true position, whatever part of the spectrum is studied, is that a slot coincident in direction with this

* Thus, if the constant angle of incidence be ϕ, and the refracting angle of each prism $2\,i$, minimum deviation is secured only for rays whose refractive index is $\frac{\sin \phi}{\sin i}$.

last-named line should be rigidly attached to an arm bearing the telescope, and should be also at right angles to the optical axis of the telescope.

In fig. 2 the actual position of the prisms is indicated. A B is the axis of the incident pencil at right angles to the fixed slot S S′ (whose direction passes through the angle D of the first prism). The double lines indicate the position of the movable slots, the last of which, O E, is rigidly attached to the viewing telescope, and at right angles to its axis, which corresponds in position with C F, the axis of the emergent pencil.

Fig 2

To obtain the greatest possible dispersion, without losing any part of the spectrum, the refracting angles of the prisms should of course be such that E G will just touch the angle D when the extreme limit of the visible spectrum at its most refrangible end is in the field.

I may add that if A B (fig. 3) be the base of any one of the prisms of the series, it will be evident from the figure that the prism A C B may be replaced by a prism E D F (A E being equal to F B, and E D, F D parallel respectively to A C, B C) without affecting the subsequent path of the *axis* of any pencil; and that for certain purposes the shortening of the path within the prism by such a change would be advantageous. I need not enter into the consideration of the other ways in which a similar shortening can be effected; but I may note that there seems nothing to prevent the equal prisms used in Mr. Browning's present arrangement from being replaced by a series of increasing prisms, represented in section by the shaded triangles in fig. 4 (the last prism of the battery remaining unchanged). If this arrangement is practically feasible, the definition would, I imagine, be appreciably improved. As the figure shows, the course of the axis of a pencil would not be so altered as to affect the path between the prisms, or the equality of the angles of incidence and emergence.

Fig. 3

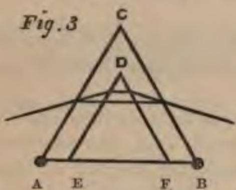
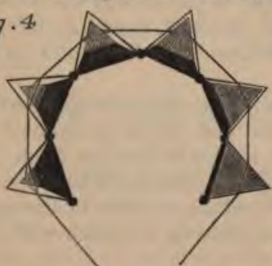


Fig. 4



Since the above was written, I have devised a plan by which

minimum deviation can be secured automatically for a battery of nine, ten, or eleven equilateral prisms. The light from the sixth prism of a series like the above (figs. 2 and 4) is received on a totally reflecting prism, from which it passes to a second battery in a reversed position. By a very simple contrivance the totally reflecting prism adjusts itself, so as always to hold a symmetrical position between the two batteries, the faces including the right-angle of the prism being always perpendicular to the light passing out of one battery and into the other. But as a long connexion, such as this arrangement involves, necessarily works imperfectly (however true theoretically), I at once double the dispersive power and halve the effective length of the connexion, by causing the light to be reflected so as to re-traverse the whole battery, and to be viewed by a fixed telescope whose optical axis is in part coincident with that of the collimator. The motion of adjustment thus has to be communicated to the totally reflecting prism, and the effective length of connexion is the same as for a single battery. I hope to be able to describe this plan in full at the next meeting of this Society. It need hardly be said that the arrangement is only useful where there is abundance of light. One secures by means of it a dispersion corresponding to that given by 22 or 23 equilateral prisms (according to the way in which the light is sent back through the battery); but the power can be reduced prism by prism to any extent which may be requisite.

The Satellites of Uranus. By Richard A. Proctor, B.A.

It has been with extreme regret that I have seen a passage in my *Other Worlds* interpreted by a Vice-President of this Society as a setting up of my mere opinion against the telescopic observations of our President. I hasten to correct this misinterpretation. In expressing the opinion that Sir William Herschel was not deceived as respects the four satellites of *Uranus* which remain unidentified, I was but doing what Admiral Smyth, Professor Nichol, and Professor Grant, had already done (simply on the score of Sir William Herschel's wonderful accuracy as an observer), I am convinced that Mr. Lassell (whose name, by the bye, I had not mentioned in the matter) would be the last to condemn this confidence in the work of an astronomer whose achievements he has so worthily emulated.

I wrote, it is true, with full knowledge that Mr. Lassell has not detected other satellites than *Umbriel*, *Ariel*, *Oberon*, and *Titania;* and with full knowledge also of the qualities of his four-feet mirror. But Sir William Herschel's observations indicate a variability in the lustre of the satellites quite sufficient to account for Mr. Lassell's want of success (so far): and though the light-gathering power of Herschel's four-feet mirror was doubtless inferior to that of Mr. Lassell's, yet it must be remem-

bered that Herschel could not see the satellites when his telescope was used (like Mr. Lassell's) as a Newtonian.*

As Professor Pritchard, at a recent meeting of this Society, spoke of himself as one of scarce three (he thought) present who had read all Sir William Herschel's papers, he will not need to be reminded that Herschel (to use Professor Grant's words) "asserted his firm belief in the existence" of these other satellites. I confess that I should require *very* strong negative evidence to force on me the conviction that Herschel was mistaken on any occasion when he expressed confidence about an observation. Writing with the remembrance of Sir William Herschel's confidence strong upon me, I said in *Other Worlds* that "one cannot read the account of Herschel's method of procedure without feeling that no amount of mere negative evidence can be opposed effectively to the positive information he has left respecting those four orbs." Is this, I would ask, to be considered as an inexcusable attack (as Professor Pritchard has implied) on our esteemed President, who has, indeed, taught us to believe in unseen satellites, *since his name will for all time be associated with the discovery of an eighth Saturnian satellite long after astronomers had concluded that but seven exist?*

Further Note on the Change of the Colour in Jupiter. By John Browning.

Since I sent in my Note in reply to the Astronomer Royal's remarks, which appeared in his Report to the Board of Visitors, I have been favoured with a copy of a paper by Prof. A. M. Mayer, of Lehigh University, Pennsylvania, U. S. As this paper appears to me to fully corroborate the substance of the three papers I have had the honour of giving to the Society, I now venture to present some extracts from it :—

"Every astronomer who, during this fall and winter, has made careful observations of *Jupiter*, must have remarked the unusual colours of his disk and belts, and the remarkable forms and mutations which the latter have frequently presented."

* * * * *

"The colour of the planet next demands our attention, and surely no one familiar with the usual tints of the belts and of the general surface can fail to remark the unusual colours which this drawing exhibits.

"*Jupiter's* disk generally is of a light yellow colour crossed by

* Professor Grant thinks it likely that Sir William Herschel refers to his 20-feet mirror, not to the 40-feet one (which was difficult to use) in his account of the discovery of these satellites. But although Sir W. Herschel does not mention specially the 40-feet telescope, it is evident from his remark about the use of the "front-view" (coupled with his original description of that method of using large reflectors) that it was his great mirror he employed. Otherwise, *indeed, the whole* matter would be infinitely perplexing.

belts of a brownish-grey tint; sometimes, though rarely, approaching a rose colour. Often the brown is entirely wanting, and the belts are merely dull streaks on a light yellow ground.

"We can best convey an accurate conception of the colouring, seen on the 5th of January, by stating the manner in which we tinted the drawing, which, when compared repeatedly at the telescope by myself and others, was found correctly to give the colours and their relative intensities.

"The whole disk was first washed over with a slight tint of yellow. The colour of the region between the equatorial belts was correctly reproduced by pure yellow mixed with crimson-lake, while the two dark belts north, and the one south of the equator were obtained by the same combination of colours, with more crimson-lake added, so as to bring the tint to approach a coppery hue. The polar shadings were correctly given by pure yellow with a slight dash of crimson-lake, which tinting was subsequently overlaid with a very thin wash of light lead colour."

Reading over the last paragraph, one might imagine it to be a description of the last coloured drawing which I have made of *Jupiter*. A copy of this drawing forms the frontispiece to Mr. Proctor's new work, *Other Worlds than Ours*. On referring to this coloured plate, the resemblance may be almost as clearly traced as by examination of the drawing. Professor Mayer used a *refractor* of six inches aperture, made by Alvan Clark.

Clapham, June 30th, 1870.

Further Remarks on the Corona. By Richard A. Proctor, B.A.

At the last meeting of this Society, Dr. Gould, of America, indicated his belief that the trapezoidal corona seen by himself and other observers during the progress of the American eclipse was in fact but the chromosphere seen under unusually favourable circumstances. He added, that the light outside that four-cornered corona appeared to shift in position, and hence he concluded that it was terrestrial.

It seems to me that if this view be admitted, the difficulty pointed out by Mr. Lockyer in the case of the corona considered generally, exists in scarcely diminished extent in the case of this trapezoidal appendage also. Estimated by most of the observers as extending fully 12′ from the disc of the eclipsed Sun, its real depth would be far more than 320,000* miles, and the pressure even at the summits of the highest prominences would be enormous.

We gain nothing, then, by Dr. Gould's supposition; though of course that does not prove it to be erroneous. But Dr.

* Even in Mr. Whipple's photograph it has an extent of fully 6′, which would correspond to more than 160,000 miles, or 80,000 miles above the highest prominences yet seen.

Curtis (whose successful photographs appear in Commodore Sands's reports of the total eclipse) remarks that he has read Dr. Gould's statements respecting the eclipse with considerable surprise. After referring to the photographic evidence, he adds, "Dr. Gould adduces as an additional argument in favour of his assumption the observation that the long coronal beams appeared to him to be 'variable,' while the 'aureole' photographed was evidently 'constant' during the time of totality. This argument, however, loses some of its force, when it is remembered that to other observers the corona appeared to the eye absolutely unchangeable, both in form and position, during the whole period of the total obscuration." He goes on to indicate the probability that Dr. Gould has mistaken a photographic effect for a real phenomenon, in this case, precisely as when he interpreted the apparent encroachment of the bases of the prominences on the Moon (a dark-room phenomenon, as Curtis shows) to "specular reflection" at the Moon's surface.

I must confess, that after a very careful study of the whole series of American observations, Dr. Gould's view appears to me to be altogether disposed of by the concurrent testimony of so many and such skilful observers.

One striking, and as yet unnoticed, piece of evidence exists in General Myers' report of the appearance of the corona as seen from the summit of White Top Mountain, 5530 feet above the sea-level. Here the same quadrangular aspect was observed as at lower levels (and in Whipple's photograph), but the rays were much longer. "The silvery rays," he says, "were longest and most prominent at four points of the circumference—two upon the upper, and two upon the lower portion—apparently equidistant from each other, and at about the junctions of the quadrants designated as 'limbs,' giving the spectacle a quadrilateral shape." He remarks that these silvery rays were "straight and massive," and extended "to a distance of two or three diameters of the lunar disc." He adds, "There was no motion of the rays."

It seems impossible to mistake the significance of these observations.

In my paper in the March number of our *Notices*, I dealt specially with the theory that the corona is due to the illumination of the Earth's atmosphere by light not affected "by any action at the Moon." Many of the arguments, however, apply equally well on the supposition that there is such action. The striking fact that at the time of central eclipse the cone within our atmosphere bounded by lines from the observer's eye to the Moon's limb, contains no light, while the cylinder within our atmosphere bounded by lines from the Sun's limb to (and produced beyond) the Moon's, contains much light, affords, I take it, absolutely convincing evidence that this light is derived from an object far beyond the Moon. For if we suppose the solar rays to get by any process within the cylinder, they should

clearly traverse the cone also. For example, assuming that a solar ray passing by the Moon's edge is deflected (by whatever cause) so as to fall within that cylinder into which (from its very nature) undeflected rays cannot pass, the deflection, in order to account for observed appearances, must carry the illumination of our atmosphere up to the above-mentioned cone, and there suddenly the illumination must cease. But the cone has no existence in nature; it is but a mathematical conception: why then should these deflected rays respect it?*

Even La Hire's theory, which De Lisle is supposed to have overthrown, seems more easily supported than one which requires a moving shadow-cylinder of air to be illuminated, while a fixed cone (*not* a shadow cone) within it remains in darkness.

It seems much more natural to regard the blackness of the lunar disc, and the relative brightness of the corona, as due simply to the fact that the Moon is an opaque body very much nearer to us than the corona.

Let me renew my statement that it is the importance of the approaching eclipse which forces me to urge now views which I have long entertained. It appears to me that if, as I hold to be the case, the evidence respecting the corona is amply sufficient to prove it to be a solar appendage, then it would be a serious misfortune if any observers were to devote their time to establishing this fact. Instead of this, I should be glad to see every moment of the short duration of totality devoted both by general observers and spectroscopists to the inquiry *what sort of a solar appendage* the corona may be. On this inquiry depend issues of the utmost interest and importance to science; the other would be a waste of time: on one question we have abundant evidence; on the other (to quote the just words† of Professor Pritchard), "wise astronomers profess their profound ignorance."

* Mr. Lockyer tells me that M. Faye expressly suggests that there is some action at the Moon, and that, according to his and M. Faye's theory, it is thus the atmosphere gets illuminated. What the nature of the action may be I have not yet heard, nor can I conceive of any which would account, however roughly, for observed facts. Mr. Seabroke's paper on the Corona in the last number of our *Notices*, assumes no such action to take place; yet it is supposed that he is there defending Mr. Lockyer's theory. On the other hand, let me note in passing, Mr. Seabroke deals with an imaginary (I had almost said impossible) eclipse, and is therefore evidently not attacking my views respecting real eclipses. His arguments are, however, mathematically accurate. At the very moment and place of second and third contacts in an ordinary total eclipse, or during the occurrence of an exact total eclipse (which could last of course but an instant), the results he educes would doubtless take place, though it would be wholly impossible to observe them. But his formulæ are wholly inapplicable to the circumstances of any actual eclipse.

† Just *per se*, though somewhat too magisterially applied as a warning to myself. I also profess complete ignorance as to the nature and condition of the material forming the corona, but it is on account of that ignorance that I am so anxious to see the skilled observers of the approaching eclipse employing the opportunity in the most advantageous manner.

The use of the Steel-Balance in estimating the Distribution of Stars and Nebulæ. By Richard A. Proctor, B.A.

It is often difficult to estimate the extent of any irregular district of the heavens, and hence the numerical statistics of star-distribution require to be supplemented by a means enabling us to deal readily with spherical areas. I propose for this purpose (and have already begun to apply) the following method. Having laid down any convenient isographic projection, let the boundaries of any space whose area we require be traced on that projection. Then let the included space be carefully cut out and weighed against the remainder of the projection, with any convenient form of the steel-balance. The area of the space thus becomes known, and the relative richess of star-distribution can at once be determined.

Suppose, for example, a certain region r contains n stars of a given order (or nebulæ, as the case may be), while the whole heavens (R say) contain N such objects. Suppose by means of the steel-balance we find the following relation between the weights w and $(W - w)$ of the regions r and $(R - r)$:

$$\frac{w}{W-w} = \frac{1}{a}\text{; then } \frac{w}{W} = \frac{1}{a+1}$$

and the diversity of distribution in the region r is to the average density of the whole heavens as $\frac{n}{1} : \frac{N}{a+1}$, or as $(a+1)\, n : N$.

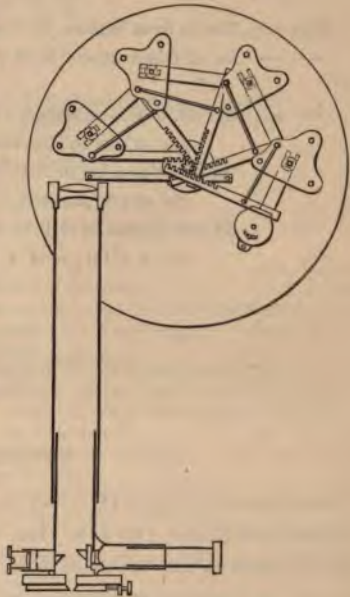
The construction of a polar isographic projection by the method described in my *Handbook of the Stars* (originally designed, I afterwards learned, by Sir John Herschel) is so simple a matter, and can be effected so accurately, that the plan described above affords a ready means of solving a number of problems connected with the areas of spherical surfaces. For example, I believe a more accurate estimate of the relation between the land and sea surfaces of our Earth could be obtained in this way than by the methods hitherto applied.

It is obviously quite as easy to compare any two celestial regions together by this method as to compare a given region with the whole heavens.

I hope at the next meeting of the Society to exhibit some results obtained by this method, which seem to indicate a surprising inequality in the distribution of stars in different portions of the heavens—I mean among those orders of magnitude within which, if the views at present accepted were true, a general equality was to have been expected according to the laws of probability.

Reply to M. de Littrow's remarks on the Automatic Spectroscope. By John Browning.

Having obtained a copy of the Number of the *Proceedings of the Imperial Academy of Sciences of Vienna*, vol. xlvii. which contains a description, with figures, of Herr von Littrow's Spectroscope, I am enabled to present the readers of the *Monthly Notices* with a copy of the principal diagram. On comparing this with the diagram of the instrument which I have contrived and described in *Monthly Notices*, vol. xxx. p. 198, I do not think any one will agree that, "the only difference between the two instruments consists in the sparing of the second telescope."



If I understand Herr Von Littrow's plan he has devised a symmetrical arrangement of the prisms, and communicated motion to each prism separately by means of a rack on a rod attached to the base of each prism, at right angles to the base, controlling this motion by means of cords. The adjustment of such an instrument must be necessarily tentative—each prism being brought right separately in the first instance; and it would be very difficult to so adjust it, that by a continuation of the rack-work motions, each prism should move with the requisite variable motion when the apparatus is put in action.

In the plan I have contrived, the prisms are so arranged that as the rod from the base of each can move freely over a common centre, and the whole of the prisms are linked together, a motion given to any part of the train of prisms must produce the desired effect, because the bases of the prisms remain always at a tangent to an inscribed circle, and, consequently, the angles between all the prisms remain the same.

I must disclaim having had any previous knowledge of Herr von Littrow's invention.

ERRATA

In Monthly Notices for June 10, 1870.

Vol. xxx. No. 8.

Page 203, line 20 from bottom, *for* Canis Venatica, *read* Canes Venatici.
— — No. of Observations of N.P.D. in 7-year Cat. (1860), *for* 8, *read* 18.
— 204, N.P.D. of Groombridge, 1830, for 1869,
for $51^\circ 20' 29''\cdot 84$, *read* $51^\circ 20' 29''\cdot 98$.
Annual Variation in N.P.D. for years 1864—1869,
for $+ 25''\cdot 72$, *read* $+ 25''\cdot 75$.
Proper Motion in N.P.D. for years 1864—1869,
for $+ 5''\cdot 71$, *read* $+ 5''\cdot 74$.

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